

Psychological Review

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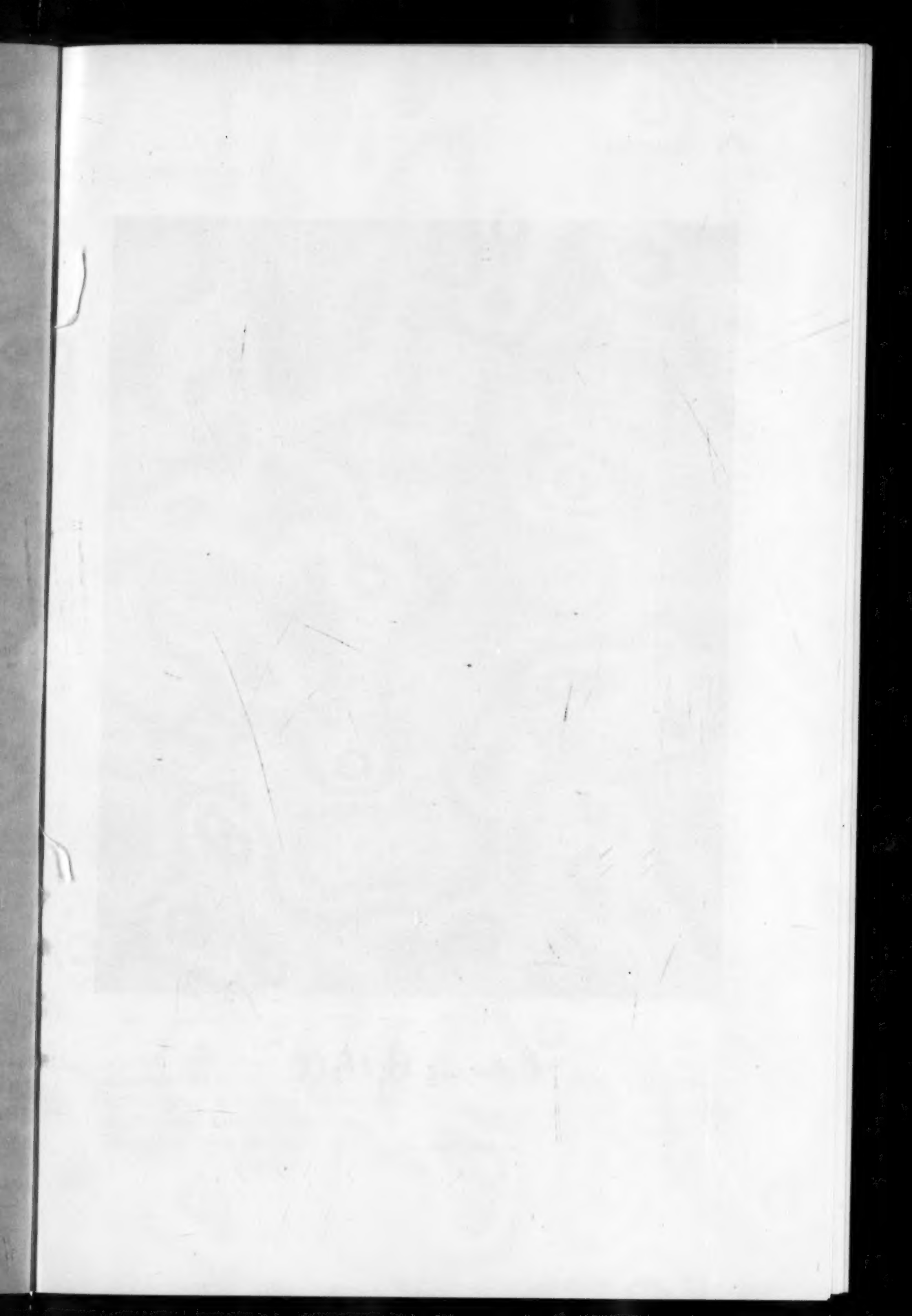
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Edwin B. Holt.

THE PSYCHOLOGICAL REVIEW

EDWIN BISSELL HOLT

1873-1946

With the death of E. B. Holt on January 25th, psychology lost one of its most colorful figures. To the younger generation he was already history. They knew his writings but they had never heard his voice, for years ago he withdrew from the active life of a teacher to a lonely spot on the Maine coast, there to bury himself in what he finally came to believe was the only thing worth while—the famous literature of the past. His was an escape from the modern trend of psychology, with, as he thought, its lack of intrinsic scholarship and insight.

Holt, although out of step with much that psychology is now doing, had considerable effect upon its development. He was one of the favorite pupils of William James, and was probably more like James than any other of James's disciples in the quality of his intellect, in his dislike of sham and outworn convention, in his independence of thought and criticism, in his brilliant conversation and originality of expression, and in his generosity in helping and encouraging little-known but promising writers. Those of his friends with whom he corresponded know the quality of thought to which I refer. An example from one of his letters may give those who did not know him an idea of what I mean. I had sent him a picture of Santayana taken in Rome after the Liberation. Holt replied:

"Amazing that Santayana seemed so little cognizant of the invasion of Rome! . . . He, so aloof and inaccessible, the one practising solipsist, moves with ease within the range of his own select and attenuated experiences, largely imaginative and verbal; but what competence would he show if *forced* for once to tackle realities? Idle question, though: even the invading hordes of Nasties could not break in on his 'living in the eternal.' A most successful, impregnable mollusc!"¹ (September 28, 1944).

Holt held James in the deepest reverence and in unflinching affection. James in turn must have enjoyed the companionship of his pupil for Holt was a frequent visitor to his house. In fact, he was treated almost as a member of the family, and saw much of the three sons, being especially intimate with young William James.

Holt was not a pragmatist, but he carried on the spirit of James's philosophy in his realism. This realistic attitude, which certainly in Holt's case was a personality trait, was back of his interest in physiological psychology and led eventually to his most important book, *Animal Drive and the Learning Process* (6).

Holt's personality was complex and often paradoxical, and his interests broad. In spite of his devotion to ex-

¹ Throughout this article quotations followed by a date are from personal correspondence.

act experimentation, he was one of the early exponents in this country of Freud's psychology. His *Freudian Wish* (5), which was widely read in America and England, was said by Freud himself to have been the best exposition of his (Freud's) theory to appear up to that time in this country.

Holt was an inspiring teacher; he was also a disturbing one. He delighted in knocking the props from under his listeners in order to make them build from the ground up. He had devoted followers, and hundreds of Harvard students who took his course in the Philosophy of Nature, and Princeton students who listened to his Social Psychology, have been influenced by him.

Holt was born August 21, 1873, at Winchester, Massachusetts, the son of Stephen Abbott and Nancy Wyman Cutter Holt. His father, who died when Holt was a young man, was a graduate of Bowdoin College and became a Congregational minister in the Bangor Theological Seminary. Holt, who held his father in great admiration and affection, used to speak of the broad views on religious questions which he held toward the latter part of his life. Holt himself was seemingly anti-religious, and was wont to make remarks on religion and clerics which often startled his listeners. Toward religion, however, as toward so many other subjects, his attitude frequently seemed contradictory, for he would inveigh against clergymen and the Church while holding in affection individual members of the Church and respecting his friends' beliefs. As I see it, the paradox is resolved by the fact that Holt was not at heart against religion, but against religious institutions, which he felt were more concerned with their power than their spiritual influence. This attitude toward 'power' was at the bottom of his dislike of all so-called leaders. He himself had decided ideas; but when he met

opposition, instead of pushing forward or even compromising, he withdrew—and the opposition knew him no more.

Holt's mother was a woman of keen intellect and a sense of humor which was like his own and which was one of the bonds between them. She was a very strong-minded, self-willed person, who worshipped him but at the same time selfishly dominated him. His attitude toward her was ambivalent. He was brilliant from early childhood, according to persons who knew him then, and his mother encouraged his intellectual curiosity. She kept a tray of silkworms for him, raising in her garden the mulberry leaves on which they fed. He was greatly interested in feeding them, watching them spin their cocoons and emerge as butterflies. His unusually broad knowledge of *flora* and his tender affection for animals were evidence of his mother's influence. Remarks such as the following frequently appeared in his letters:

"We are having a grand blizzard tonight; thick snow and a high wind. Our four sparrows will need special attention tomorrow" (February 11, 1944).

Whenever he could, he kept a cat and a dog, and a stray animal was always welcomed. He had a great respect for their intelligence and would not say anything in their presence which he thought would hurt their feelings. It might be added that it was this deep-seated love of animals that made him so critical of much of animal experimentation.

At about eight years of age he had a toy theater for which he planned scenery and costumes. This interest for the theater continued through his life. He made toy theaters for the children of his friends, and for a time helped make the scenery for Baker's Forty-Seven Workshop. He was always keenly alive to

and intelligently critical of the developments of the professional stage.

Holt went to school in Winchester, and in 1892 to Amherst. At the end of his freshman year, he transferred to Harvard. He never kept up his Amherst contacts. In 1896 he received his Bachelor of Arts degree, *magna cum laude*, from Harvard. He attended the Harvard Graduate School of Arts and Sciences from 1897 to 1898. When the Spanish-American War broke out, he joined Battery A, 1st Regiment, Massachusetts Artillery, U. S. Volunteers, and was with the outfit from May 6 to November 14, 1898, being stationed successively at Fort Warren, Mass., Nahant, Mass., and Fort Pickering, Salem, Mass. When, as a Harvard student, he first joined up, he was considered a softie by his hard-boiled companions. It was not long, however, before he had out-distanced them in the use of those words which have always been a large part of the vocabulary of soldiers. It was during this period, then, that he acquired and never lost that picturesque form of expression so familiar to his friends and so astounding to his new students.

After the war he attended the Medical School of the University of Freiburg, Germany, for one year. On returning to the United States he went to Columbia, where he received a Master of Arts degree in 1900. Then back to Harvard for his doctoral degree in 1901. The men who influenced him most at Harvard were James and Münsterberg. He liked neither the idealist philosophy of Royce nor the ethics of Palmer. Münsterberg was Director of the Psychological Laboratory, and he had Holt appointed instructor, a rank he held from 1901 to 1905. Early in his teaching career he received an offer from England. As he wrote, in learning of S. Alexander's death:

"I am very sorry that S. Alexander has gone. Every one spoke of him as a most

loveable gentleman. I never saw him; but in one of the very first years of my teaching at Harvard he offered me, through Wm. James, an opening at Manchester. And except for my Mother's need of me, I should unquestionably have gone" (September 22, 1938).

It is interesting to speculate what would have happened if Holt had accepted this offer. His scholarship in the Old World tradition, his originality and his interest in the philosophical problems of psychology were greatly liked by the English. His *Concept of Consciousness* appealed even more to the British than to his fellow countrymen, and his *Freudian Wish* at least as much. In England Holt was, in the old days, one of the best-known and most respected of American psychologists. He would have been in a congenial atmosphere because it would have been one of warm appreciation. He needed the encouragement he would have received, and it is quite probable that his production would have been even greater.

In 1905 he was made assistant professor, a rank he held until he withdrew from Harvard in 1918. In the academic year 1910-11 he was Acting Director of the Psychological Laboratory during the absence of Münsterberg in Berlin. Holt was for many years the real guide of the graduate research. Münsterberg assigned most of the problems and criticized the final results, but Holt helped the students set up their apparatus and saw to it that the research was carried out properly. He was generous with his time, but meticulous and exacting. No careless work ever got by him, and many of his graduate students have repaid him with unflinching loyalty for the benefits they derived from his guidance. He had considerable manual dexterity, and liked to work with instruments. Also it was part of his motor theory that one should use his hands as well as his 'mind.' He was

very neat, and spent much time in keeping the Laboratory in order. It was a familiar sight to see him at his desk, painstakingly painting and lettering little tin boxes to hold nails, pins, and the like.

There is little doubt that James had the most influence on his thinking. In the early days he was also very close to Münsterberg, whose theory of response, though pleasing to Holt, suggested a very different approach. He was an intimate friend of the Morton Prince family. He and Morton Prince had pleasant times in each other's company although they were in only partial agreement on important problems of psychology. Of the younger men in the department, Holt saw most of Yerkes and Perry. For many years he and Perry lunched together in the laboratory and discussed philosophical questions relating especially to their growing interest in a realistic approach. He organized and ran a small discussion and social group called the "Wicht Club" which met frequently and played a large role in his life for a number of years. The members were R. P. Angier, W. B. Cannon, E. B. Holt, Gilbert Lewis, H. W. Morse, R. B. Perry, G. W. Pierce, W. H. Sheldon, E. E. Southard and R. M. Yerkes. For several years the publications of the group were collected, and under Holt's direction bound in handsome volumes entitled *Was Wichtiges*. This was an unusually inspiring group of intimate friends, all of whom became leaders in their respective fields. There is no doubt that, in the give-and-take of these friendly discussions, Holt increased his knowledge of the sciences and of philosophy, and sharpened the critical faculty which was one of the most evident characteristics of his mind.

During World War I Holt was too old to engage in active service. He therefore volunteered as a 'Dollar-a-Year

Man' in Washington, where he worked in one of the Government offices.

Holt made it clear to his friends about this time that he was only remaining at Harvard because he had to take care of his mother. As a matter of fact, he had his letter of resignation already written and the night his mother died in January, 1919, he mailed it at the corner post-box. President Lowell, who was always very fond of him, called me to his office and asked me whether there was anything he could do to keep him; he was willing to offer him a professorship and the directorship of the Laboratory. Holt, however, never could be dissuaded when he had once made up his mind.

For the next few years he lived in various parts of New England, California and British Columbia, generally with a friend, sometimes alone. In 1926 he came to Princeton as Visiting Professor in the Department of Psychology, where he remained ten years. During most of this period he taught only in the Spring term, when he gave Social Psychology which was frequently voted by the Seniors one of the most interesting courses. He was an unusually good teacher. His voice was pleasant, his language distinguished, his exposition clear, his interest sharp and his criticism often caustic. In his course he started with the reflex circle and conditioning, and gradually developed his description and criticism of social institutions. Most of his precepts were in the evening; often students would stay through two precepts and on into the night, and accompany him home about one o'clock in the morning.

In 1936 he felt tired and decided he did not want to teach any longer. He therefore returned with his devoted friend, George Bernier, to Maine, where he eventually built a house at Tenants Harbor, and worked on the second volume of his *Animal Drive*. But his heart

was no longer in his work. In April, 1944, he wrote, "I doubt that the contemporary crowd cares an iota for any of the topics in which I am interested. Nor do I care for any in which they are interested. That is what it means to grow old." The war also depressed him greatly. In July, 1940, he wrote, "Everything is topsy turvy and disheartening. I can see nothing but the complete collapse of Western civilization. How fortunate that you and I are not exactly young." In November, 1944, he caught a "terrific cold" in Boston. Back at Tenants Harbor he began to suffer from the muscular pains which kept him in bed until his death.

Holt's list of publications is not long. There are 30 articles and books under his name in the *Psychological Register* of 1932, three of which are there by mistake (a Vassar student, Elizabeth B. Holt, was one of the co-authors of these articles). Holt wrote slowly and with great care. His style, however, was easy, clear and graceful. Words interested him. He had an unusually wide vocabulary and an uncanny ability in selecting exactly the right word or phrase to express his meaning. He disliked unusual words if a common one would do, and his expressions were direct and to the point.

His doctor's dissertation was on 'Eye-movement and central anaesthesia.' It was published as a monograph in 1903 (1) and also appeared as the first article in the first volume of the *Harvard Psychological Studies*. He concluded "that voluntary movements of the eyes condition a momentary, visual, central anaesthesia" (p. 45). This field of research interested him for about six years. In 1910 he started collaboration with Marvin, Montague, Perry, Pitkin and Spaulding on a program of philosophy based on the principle of realism (2). This group—known to their friends as the Six Little Realists—

published *The New Realism* (3) in 1912. Holt's chapter was 'The Place of Illusory Experience in a Realistic World.' His problem as a realist was to explain secondary qualities, optical illusion, illusion of thought, errors of thought, etc. He ends his essay with "A consciousness is a group of (neutral) entities to which a nervous system, both at one moment and in the course of its life history, responds with a specific response" (3, p. 373). This sentence is significant in that it is prophetic of what was to appear in his *The Concept of Consciousness* (published in 1914 and dedicated to Münsterberg), and also of his motor theory. *The Concept of Consciousness* (4) is primarily philosophical. Much that he wrote then he has since denied, especially the neutral entities. It is also interesting to note his arguments for Münsterberg's action theory as against McDougall's drainage theory. In the light of what we now know about his motor theory of consciousness, we can see a certain influence that Münsterberg had in directing his thought. Holt liked some of Münsterberg; he liked McDougall's physiological psychology and his social psychology. He also was enthusiastic over Watson's behaviorism, but was cold towards these men's later writings.

In 1915 appeared the most popular and most widely read of Holt's books: *The Freudian Wish and Its Place in Ethics*. He applied Freud's principles to explain behavior but in such an original and delightful manner that many persons spoke of the book as 'the Holtian Wish.' He wrote also of the physiological basis of wishes, in which he introduced examples of conditioning, though not using the word. He was troubled because he could not explain how reflex paths are integrated into complicated behavior—one of a number of similar problems that engaged his attention and were more fully un-

derstood by him later. In the same year he published a paper 'Response and Cognition' which was reprinted at the end of *The Freudian Wish*. It contains a discussion of realism—the refutation of the soul and subjectivism, and the importance of specific response in cognition.

Freud was so very close to Holt's heart that he refused to write an article on him for the *PSYCHOLOGICAL REVIEW* because there were so many 'grave defects' in Freud's theory which he did not care to describe publicly. As to the features of the theory which he liked, Holt wrote:

"The points for which I venerate Freud are his fine empirical observations in the earlier part of his career; his substituting the dynamic 'wish' for Janet's non-dynamic and futile 'dissociation'; and lastly, his supreme integrity, in science and in everything. On the second point (where Freud helped me so greatly toward a motor psychology) I said in 'The Freudian Wish' all that I could now say. And I think that few readers understood or cared; since so few have been led on to a motor psychology" (November 14, 1939).

Holt was supposed to revise James's *Psychology* after the latter's death. For years he pondered the problem. At first it seemed difficult. Finally he found it impossible since psychology had advanced so rapidly during this period that a new book was necessary, and *Animal Drive and the Learning Process*, in 1931, was the answer. It is the culmination of his thinking and lecturing through many years; with much of it his students in Social Psychology were already familiar. He builds up behavior from the reflex circle and circular response, through chain reflexes, adience response, motor theory, to integration. The term 'reflex circle' has given students difficulty. In fact, he wrote on April 8, 1943: "Unless ——— is a really intelligent person he would

better not touch the reflex-circle; it ramifies too dreadfully." However, in a previous letter (March 24, 1943) he sent me a description of both reflex circle and circular response, which is clarifying:

"The term 'reflex circle' and 'circular reflex' have caused much trouble. They are so much alike that they remain confused in the mind in spite of explicit definitions. Whether I could have explained them more clearly I really don't know. 'Circular reflexes' (Sherrington) are only one (though a numerous) class of 'reflex-circles'; a special case of r.-c. Many reflex-circles are *not* 'circular reflexes' (in Sherrington's sense, of course).

"Ariëns Kappers' principle of neurobio-taxis is the physical *cause* of all these phenomena, but it operates unseen, at the microscopic level.

"Now Bok's 'reflex-circle' principle (or law, if you prefer) is:—If the contraction of any muscle causes any sense-organ to be stimulated, then that sense-organ will soon acquire a neural connection to that muscle. The grasping reflex and auditory-vocal response are cases of this. The sense-organ involved in the former is the tactile surface of the palm and finger-tips, in the latter it is the cochlea; *exteroceptors* both.

"Circular reflex is that special case of reflex-circle, merely, in which the *sense-organ involved* is a *proprioceptor*.

"I tried to make this explicit and clear on pages 37–8 of *Animal Drive*, and again with the first sentence on page 44."

"You ask: 'Isn't the functional connection in the central nervous system the same thing as the reflex circle?' No! The essence of the reflex-circle is its *genesis*. The 'law' that I gave on the previous page is a *genetic formula*; and that is why the reflex-circle leads us on to a strictly *dynamic* psychology. It is by this genetic formula that the connections in the central nervous system become moulded to, and subservient to, our own *external* anatomy and to the objects of the outer world. It is on the reflex-circle principle that *such* func-

tional nerve paths are formed as will produce significant responsive behaviour."

In 1935 Holt published a short essay entitled 'The whimsical condition of social psychology and of mankind' (7) in which he first reviewed the concepts of the reflex circle and of adience. This latter response is the basis of self-interest, according to Holt. From here it is an easy transition to his favorite topic of leaders, especially political and other institutional despots. It is a very Holtian paper and particularly interesting in view of the present prominence of the concept of ego-involvement.

Holt was very much interested in language. His guiding hand is much in evidence in Latif's thesis (9), but he has not written much himself on the subject. He wrote me at length on the topic on February 9, 1941, however, and inasmuch as the letter gives us his latest views, it seems profitable to quote it here.

"May I spill a few half-baked reflections about thought and language? Pareto had a good impulse when he tried to distinguish between instincts (& all non-linguistic behaviour), or *Résidus*, and linguistic behaviour (thought-speech), or *Dérivations*. But I do not see that he threw any real light on the matter. Mauthner, with the same dichotomy in mind, declared that it is precisely the distinction between the Kantian *Verstand* and *Vernunft*. The Reason, for him, is essentially *verbal*; and perhaps nothing else. I do not agree to the 'nothing else,' but I think the remark is extremely sagacious, and significant. However, even Mauthner does not take us much further on just this point.

"In my *An. Drive* I tried to say something about the origin and mechanism of non-linguistic behaviour (the *Verstand*). The transition to linguistic behaviour is primarily along the classic lines of the child's picture-book. The word (heard or read) starts us off towards the object (if in sight) which the word 'means' or/and sets us to re-creating a ('subjective') image

of the object 'meant.' (Such is the *motor* Meaning of Meaning.) And vice versa, the object as stimulus, or the subjective image as proprioceptive stimulus, sets us to utter the name or appropriate descriptive words.

"As soon as even a small vocabulary exists everything favours an enormous social traffic in words and their attendant subjective images alone and quite apart from immediate contact with the *things* in question. With this begins an endless process of dislocation and transmutation of meanings, held but little in check by rare and spasmodic renewals of contact with the things. *Die Vernunft!* Reason-logic, 'thought,' verbalism metaphysics, etc. The great differences coming in here (Pareto's and Mauthner's dichotomy) are because perception and observation are almost wholly in abeyance, and are superseded by purely associative ('mental') phenomena. Almost everything in this realm of *Vernunft* is *artifact*. And Vaihinger in his *Als Ob* only scratched the surface.

"That fact was Gruppe's revelation; and it is significant that Gruppe was a philologist. (Both Vaihinger and Mauthner took their lead from Gruppe.) These artifacts are distortions; some of them may be useful. With them the motor theory has its best fun.

"Of late I've been mulling over this:— In the 3d part of Latif's thesis is a section on thought along a linear dimension (with a reference, I believe, to C. K. Ogden's book on *Opposition*). The two ends of such a dimension usually have names and figure as opposites (*rich-poor, large-small, etc.*). Gruppe pointed out that such opposites are 'correlatives,' neither one having any meaning except relatively to the other; *i.e.* (for me) 'thought' re-creates the dimension. A moment's introspection on the pair *up-down* will illustrate this. Further, said G., vastly more predicates than we ever supposed are such correlatives (*e.g. animate-inanimate, existence-nonexistence, objective-subjective*). If one is predicated and the other totally excluded, nonsense results; as in the idealist's pan-subjectivism.

"In a diagrammatic thinking (*e.g. Herbart's* psychology) such 'dimensions' often

provide the co-ordinates of the diagram. Thus, Herbart's system is essentially a diagram, an image of a surface of water, with air above and water beneath. The pair *conscious-subconscious* is the vertical co-ordinate of the diagram. The sharp demarcation at the water-surface is a damaging artifact.

"All this may be very boring. It interests me because I think I begin to discern the very warp and woof of the Verunft. Anyhow I'll desist."

Finally, mention should be made of an article entitled 'Eight steps in neuro-muscular integration' (8) which he contributed to a symposium dedicated to Professor J. S. Beritoff. In it one will find the high lights of Holt's most mature thought, and will realize how much he contributed to the explanation of behavior, both individual and social, in terms of neuro-muscular processes.

In relation to social and political problems, it is difficult to classify Holt. Just as one thinks he has him neatly pigeon-holed, he slips out. He never acted true to any form, and he was less given to stereotyped thinking than any other person I have known. In some ways he was a liberal—certainly in his attitude toward most racial problems. Toward economic questions he was a rock-bound conservative; toward politics also. He hated 'leaders' and always thought them fundamentally self-seeking. He avoided anyone who had a tendency to 'command.' Above all he was an individualist and a staunch believer in individual freedom.

Personalities like Holt's are rare in science. Seldom does one find such a combination of clearly-focussed think-

ing and broad human interests in art, people and politics. In brilliancy of conversation he had few rivals. His writings, on whatever subject, were always original, interesting and inspiring. As scientist and man of letters the unique place he held in psychology cannot be filled.

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REFERENCES

1. HOLT, E. B. Eye-movement and central anaesthesia. *Psychol. Rev., Monogr. Supp.*, 1903, 4, No. 17, 3-45. Also in *Harvard Psychol. Stud.* (H. Münsterberg, Ed.), 1903, 1, 3-45.
2. —, MARVIN, W. T., MONTAGUE, W. P., PERRY, R. B., PITKIN, W. B., & SPAULDING, E. G. The program and first platform of six realists. *J. Phil., Psychol., Sci. Meth.*, 1910, 7, 393-401.
3. —. The place of illusory experience in a realistic world. In *The new realism*. New York: Macmillan, 1912.
4. —. *The concept of consciousness*. London: Allen, 1914.
5. —. *The Freudian wish and its place in ethics*. New York: Holt, 1915.
6. —. *Animal drive and the learning process, an essay toward radical empiricism*. New York: Holt, 1931.
7. —. The whimsical condition of social psychology, and of mankind. In *American philosophy, today and tomorrow* (H. Kallen & S. Hook, Eds.). New York: Lee Furman, Inc., 1935.
8. —. Eight steps in neuro-muscular integration. In *Problems of nervous physiology and of behavior* (A symposium dedicated to J. S. Beritoff). Tiflis, U. S. S. R.: Academy of Sciences of U. S. S. R.—Georgian Branch, 1936.
9. LATIF, I. The physiological basis of linguistic development of the ontogeny of meaning. *PSYCHOL. REV.*, 1934, 41, 55-85 (Part I); 153-176 (Part II); 246-264 (Part III).

ON THE NATURE OF FEAR

BY D. O. HEBB¹

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In the course of an experiment dealing with individual differences of behavior among chimpanzees, observations of fear were made which held an immediate interest. Besides extending the information concerning the causes of anthropoid fear which is provided by the work of Köhler (23), Jacobsen, Jacobsen and Yoshioka (17), Yerkes and Yerkes (42), Haslerud (10), McCulloch and Haslerud (31) and Hebb and Riesen (14), the new data brought up again the question of mechanism. Analysis of the behavior leads, in the present discussion, to a review of the whole problem and an attempt to formulate an hypothesis of the causes and nature of fear.

NATURE OF THE DATA

Validity and reliability. The validity of naming fear in chimpanzees, or recognizing something in animals which can be identified with fear in man and the reliability of naming have been discussed elsewhere (13). There it was shown that the recognition of emotion in an animal is possible in the same way as in another human being. Fear named in an animal means either that there was actual avoidance of some object or place, or that the observer inferred from incidental behavior ('associated signs') that avoidance was imminent and likely to appear with further stimulation. When such inferences are made with confidence by experienced observers, it appears that they are valid and reliable, the criterion being the animal's subsequent behavior.

Definition of fear behavior. The symbol 'W,' for withdrawal, was re-

corded when the animal actually moved away from a test object in such a way as to show that he did not move by coincidence, but was responding to the test situation. The evidence was of several kinds: (1) when change of position of the test object produced a corresponding movement of the animal, maintaining his distance from it; (2) when the original movement was abrupt and coincided exactly with the appearance of the test object; (3) when there was coincident evidence of unusual excitation, such as erection of hair, screaming, threatening gestures directed at the test object, or continued orientation of gaze at the object while moving directly away from it. On occasions one of these three forms of evidence alone, if exceptionally clear, might provide the basis for an entry of 'W' in the record; usually, at least two were present before the entry was made. In many instances the experimenter was certain that an animal would be afraid to approach the test object, but did not record his opinion since the formal behavioral criteria were not met.

EXPERIMENTAL METHOD

The experimental procedures were part of a study of individual differences of emotionality and temperament, and not planned to meet the problem of defining the adequate stimulus to fear. Thus the range of test objects was limited, and the order in which they were presented does not permit an exact comparison of the excitatory value of each.

Test objects. The test objects were representations of animals, from reptile to man, varying considerably in completeness and verisimilitude. They fall

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in three classes: primate objects, pictures of primates, and nonprimate objects. It was not expected that the pictures would induce fear—they were used for another purpose—but they were presented in the same way as the other objects and consequently are useful as control material.

Primate objects. There were 9 objects 'representing' primates. The responses to these are the main interest of the study.

(1) An adult chimpanzee head, three-fifths life size, made of papier-mâché and painted to appear reasonably lifelike.

(2) An unclothed doll representing a human infant, one-half life size.

(3) An infant chimpanzee's head and shoulders, nearly life size, modelled in wax and painted—about as lifelike as the adult chimpanzee head.

(4) The cadaver of a chimpanzee infant, newborn, fixed in formalin.

(5) A lifelike full-sized human head from a window-display dummy.

(6) The skull of a 5-year-old chimpanzee, with movable jaw controlled by a string.

(7) The roughly mounted skin of a spider monkey, with head and shoulders movable by means of string.

(8) An unpainted plaster of Paris cast of the visage of an adult chimpanzee without the ears or the rest of the head, made from a death mask.

(9) The cured and flexible hide of a 5-year-old chimpanzee, somewhat denuded of hair; the proportions of the skin about the head and face were distorted out of recognition, but the hands and feet were recognizable.

The pictures are not described in detail, since they are important here only as 14 emotionally unexciting objects, presented in the same way as the others.

Nonprimate objects varied greatly in verisimilitude, from a careful replica of

a snake to a 'bug' which was a rectangular block of wood on coiled-spring legs.

(1) A dog's head and forequarters, of cloth, slipped over the hand and manipulated from inside with the fingers; this common toy is surprisingly lifelike in its movements.

(2) A model of an imaginary white 'grub,' 4 inches long, with long white legs.

(3) A grub identical in proportions and color, one-third as large.

(4) A rubber tube, $\frac{1}{2}$ inch diameter, 24 inches long, with a roughly carved wooden snake's head at one end; so mounted, with string inside the tube, that it could be given a snakelike movement without apparent external agency.

(5) A rectangular wooden 'bug,' 6 inches long. It was capable of an oscillating movement, since it was mounted on six coiled-wire legs, and had oscillating 'antennae.'

(6) A 'grasshopper,' a mechanical toy with moving legs.

(7) A similar 'turtle.'

(8) A rubber dog, $3\frac{1}{2}$ inches high.

(9) A brightly colored cloth dog, 7 inches high.

(10) A painted wax replica of a coiled 24-inch snake.

Procedure. Test objects were presented to the animals while they were in their own living cages. The animal or pair of animals was first brought to the front of the cage by an offer of a small amount of food. The hinged top and front of the presentation box (which was wheeled from cage to cage) was then lifted, exposing one test object to the chimpanzee. At the end of 15 seconds the test object was set in motion, if it had movable parts; if not, it was moved forward about 6 inches nearer the animal. The presentation box was closed at the end of another 15 seconds; total exposure was thus 30 seconds. The box had three compartments, and three objects were shown in

succession on each experimental period, once or twice a week. The objects were shown to all animals in the same order with the same time intervals.

EXPERIMENTAL RESULTS

With a fixed order of presentation to all subjects, there is a probability that the serial position of a test object will affect the degree of response to it, either by negative adaptation or cumulative effect. There were marked indications that such effects occurred. Some animals apparently learned that the test objects, at first terrifying, would not move out of the presentation box; others began to show fear in the later trials before the box was opened at all.

The total number of animals making fear responses to any object, therefore, is not a wholly satisfactory index of its relative effectiveness in provoking fear. However, there is evidence that the amount of such error is limited. In each group of three objects one or more pictures were included. The number of

avoidance responses was consistently low for these pictures, while remaining high for objects, such as dog or snake, known from the work of others (Yerkes, Haslerud) to be fear provoking. This means that transfer or generalization effects were limited. Also there was no sign of a steady increase or decrease of fear responses as the experiment progressed. The animals' responses were highly selective. Preliminary observations, and tests made after the completion of the experiment, also make it clear that such objects as a head without the body attached are in themselves capable of eliciting panic, and that the number of fear responses to human or chimpanzee head, recorded experimentally, is not due to an association of these test objects with the others.

The table presents the number of fear responses to each test object, separating primate, pictorial and nonprimate objects. The table gives the order of presentation and also shows which three objects were grouped together for each

NUMBER OF ANIMALS (FROM A TOTAL OF 30) MAKING FEAR RESPONSES TO 'PRIMATE' TEST OBJECTS, 'PICTURES,' AND 'NONPRIMATE' OBJECTS

(M) indicates that the object was put in motion during the presentation.

	Primate	Picture	Nonprimate
Test I	Adult ape head: 7	picture: 0	dog head (M): 10
II	Doll: 4	picture: 1	large grub: 3
III	Infant ape head: 3	picture: 2	rubber tube (M): 3
IV	Infant: 1	picture: 1	wood-wire bug (M): 3
V	_____	picture: 0	mechanical
VI	Human head: 12	picture: 1	grasshopper (M): 4
VII	Skull (M): 24	picture: 0	rubber dog: 5
	Monkey (M): 16	picture: 4	_____
VIII	_____	picture: 4	small grub: 2
			mechanical
IX	Cast of ape visage 14	picture: 0	turtle (M): 8
X	Ape hide: 5	picture: 0	cloth dog: 8
		picture: 0	_____
XI	_____	picture: 0	
		picture: 0	Cast of snake: 21
	Total = 86	Total = 13	Total = 67
	Mean = 9.6	Mean = 0.9	Mean = 6.7

test period. It is assumed, partly on the basis of evidence not presented here, that what particular pictures were used is irrelevant and that the number of animals avoiding the pictures is an index of the 'spread' of fear from exciting to neutral objects. From a total of 30 animals the mean number making fear responses to each primate test object was 9.6; to pictures, 0.9; to nonprimate objects, 6.7. These scores, it must be remembered, are the number of actual overt withdrawals which met the criteria set up in advance for a definable fear response. They take no account of signs of fear which were peculiar to an individual animal. Also, they are the number of such responses made while animal and test object were separated by a stout wire mesh. Tests in other circumstances show a higher percentage of avoidance, and show also that the relative effectiveness of two objects as causes of fear may vary somewhat according to the mode of presentation. In the conditions of the experiment, the following are the most effective stimuli, in descending order: *skull* (with moving jaw); painted wax *snake*; *monkey* (with moving head); plaster cast of *chimpanzee visage*; and *human head*. Least exciting are, in ascending order, chimpanzee infant; small wax grub; infant chimpanzee head; large wax grub; moving / rubber tube ("snake"); and moving wood-and-wire 'bug.'

SUPPLEMENTARY OBSERVATIONS

The chimpanzee's fear of toy animals and snakes is of course well known (23, 42). The data which are new and which were the occasion of this report are those showing that the chimpanzee is excited by, and avoids, parts of chimpanzee or human bodies. It was evident that such a conclusion had important implications, and that further observations would be desirable as a con-

trol of the data. Control observations, accordingly, were made after the formal experiment was completed. Their purpose was to discover whether some peculiarity of the actual experimental objects, or some detail of procedure, might have been the true cause of fear; or whether the behavior falls into a more general class related to the common human avoidance of a mutilated face and of dead bodies.

Preliminary experiments had already shown that all the adult chimpanzees were excited at the first sight of a chimpanzee head modelled in clay and carried in the hand from cage to cage. A majority showed avoidance, which was outright panic in five or six of the thirty subjects. In the supplementary observations an unpainted plaster cast of the clay model, and also an actual head from a dead chimpanzee, produced definite avoidance.

With different presentations the results were essentially the same, although intensity of response varied, in part with adaptation to sight of so many similar objects. Avoidance was observed when a head was carried by hand; when it was exposed by removing a cloth or opening a box; and when the head was first put in the chimpanzee's cage and the animal admitted afterward. In another observation the head was placed behind a small ledge, so that the actual termination of the neck was not visible (although the chimpanzee 'knew' from familiarity with the cage in which the test was made that there was no space large enough for a body beneath the head). The chimpanzee was then admitted from a detaining inner room from which none of the preparations could be seen. A marked fear response occurred immediately, before the lapse of enough time to make the unresponsiveness of the head abnormal. Thus lack of movement in the

test object did not determine the fear, nor yet an actual perception of the termination of the neck.

A painted human eye and eyebrow (sawn from a plaster manikin's head) produced marked avoidance.

Finally, observations were made with anesthetized chimpanzees as stimulus objects. Four adults were shown an anesthetized infant, two years old, carried by two members of the staff. The infant was recovering from nembutal, and made some spontaneous movements of an arm and hand. Three of the four adults were very excited and one at least afraid, in spite of the fact that they had often seen young chimpanzees being carried by the staff. A more deeply anesthetized adult was taken on a low, flat, two-wheeled barrow up to the cages of nine of the adults. Definite fear was shown by six, aggression (possibly related to fear) by two others, and the remaining animal was almost certainly afraid but remained at a distance without showing definable avoidance.

The fear evoked by a detached face or head in the formal experiment, therefore, was not a product of some uncontrolled detail of procedure or of the construction of the test objects. Any of a number of related stimuli have the same effect, in a number of situations.

From the data it appears that *either* lack of responsiveness in a whole animal, *or* an evident lack of a body when the head or part of the head is seen, can determine the fear. The first conclusion depends on the observations with anesthetized animals as stimuli. The second follows from the fact that avoidance of an isolated head was immediate and certainly was not delayed long enough for an unusual unresponsiveness, as such, to have become apparent before fear occurred.

SPONTANEITY OF THE FEAR

The fears observed must also have been spontaneous,² and not conditioned by some association of the test objects with a more primitive source of fear such as pain. This is shown by the following considerations.

There are two ways in which fear of a detached head or an anesthetized animal could be due to learning. Fear might occur (1) because the subjects recognized part of a whole which they had learned to fear in the past, or (2) because of an earlier association of a class of objects (detached heads, abnormally unresponsive chimpanzees) with a more primitive cause of fear.

(1) The first explanation can be ruled out. The dummy human head represented an ordinary young man whom the adults of the colony might have teased or injured, as they often tend to do with strangers whose general appearance is similar to that of members of the laboratory staff, but whom they would not have feared. The cast of a face was a faithful replica of the chimpanzee Lita's, made from a death mask. She had died not long before the experiment began, and certainly would not have been a source of fear to any of the other chimpanzees with cage wire intervening, as in the conditions of the experiment. The anesthetized infant in his normal state would not have been feared by an adult; and the anesthetized adult who was used as a stimulus object was Don, who is dominated by almost all of the other adults of the colony. The test object which aroused fear therefore did not do so because it was recognized as part of a whole which

² The term 'spontaneous' is used here to mean that the fear is not built up by association, as a learned response. The term is not synonymous with 'innate' since there are definite factors of past experience involved, as will appear in the later discussion.

in its normal completeness would have caused the response.

(2) The second possibility to be examined is that an association had been formed earlier between the class of stimulus objects and some event such as pain, loud noise, or a fall. For animals born in the bush and captured when their mothers were killed this is a real possibility. But nine of the adolescent and adult subjects of the experiment were born and reared in captivity and definitely had no opportunity to make such associations. None of these had seen a detached human or chimpanzee head; a few of them had seen a dead chimpanzee, but no more primitive cause of fear would be associated with the sight. The nine animals who are known not to have such associations showed on the average rather more frequent and stronger avoidance than the remaining twenty-one animals.

These facts require the conclusion that the fears discussed are spontaneous. Further support for the conclusion is found in the behavior of human beings.

HUMAN AVOIDANCE OF MUTILATED AND DEAD BODIES

Human emotional responses to the dead and to such things as the sight of a major operation or of a badly mutilated face cannot reasonably be attributed to conditioning. The responses tend to be strongest on the first experience, which eliminates direct conditioning as an explanation and requires the supporting assumption of a preliminary verbal conditioning which forms the whole basis of the response. But if avoidance were so readily established, with no innate tendency toward fear of the conditioned stimulus itself, one could easily keep children from playing in dangerous places or train adults to drive automobiles carefully—by verbal

instruction alone. This is the essence of Valentine's (39) brilliant criticism of Watsonian theory and my rejection of the explanation by conditioning rests upon his argument. What he did was to show how easy it is to condition fear of some things, how hard with others, and thus demonstrated the existence of emotional susceptibilities which are the basis of spontaneous and almost spontaneous fears.

Watson's (40) theory of fear has rightly had a profound effect upon psychological thought, and a radical departure from his ideas is not easily accepted. Yet the present situation is that the theory has been demolished, with no good substitute in sight. Jones and Jones' (22) experiment on the human fear of snakes constituted a strong and radical attack on Watson's theory. The evidence adduced by Valentine (39) reinforced the attack with evidence from a variety of fears. He has shown that there is a wide range of situations, not easily defined or classified, which have some tendency to evoke human fear. Finally, Hebb and Riesen (14) have shown the existence of a spontaneous fear of strangers in infant chimpanzees, where the customary appeal to the subject's unknown past experience is impossible and the explanation by conditioning ruled out.

Watson's work, consequently, provides no more than a starting point in determining the causes of fear and gives no reason to reject the conclusion that human fear of dead or mutilated bodies is spontaneous. The conclusion is also not affected by the fact that an almost complete adaptation to such stimuli is possible, nor by the fact that some persons may not have an emotional disturbance at their first sight of an operation, autopsy or dissection. It has sometimes been assumed that if a fear is not general it must have been learned by those who do have it: that an innate

fear should be found in all persons. This argument of course is quite invalid, in view of the existence of individual genetic differences, and it has been seen that some of the chimpanzee fears discussed in this paper are not found in all animals and yet cannot be ascribed to learning.

The evidence therefore is that both in man and in chimpanzee there occur spontaneous fears of mutilated and unresponsive bodies. The chimpanzee knows nothing of anesthesia, has no abstract conception of death, and presumably may confuse a model of a head and the real thing. Considering the intellectual differences between the species and the extent to which man's behavior is influenced by speech, one must say that human and chimpanzee fear susceptibilities, with dismembered or inert bodies as stimulus objects, are remarkably similar. In this fact there is further support for the idea that such fears are spontaneous and not associative or conditioned.

So that this conclusion will be seen in the proper perspective, the reader is reminded that the importance of learning is not minimized. There are essential factors of past experience in the fears which have been discussed; and the hypothesis which is to be presented lays a good deal of emphasis on learning as an element in the development of any fear.

CENTRAL VERSUS SENSORY FACTORS DETERMINING FEAR

The first step in an analysis of fear is a better definition of the problem and of its relation to other psychological investigations.

It should be specified that the problem is not simply that of the subcortical motor integration of fear behavior. The earlier studies of Bard (2) had the effect of concentrating attention on the hypothalamus, but it is now evident

that more must be taken into account. The analysis by Lashley (24) and Masserman (32) has limited the emotional functions of the diencephalon to a motor integration. More recently, Bard (3) has described rage in a cat lacking *only* the hypothalamic region which he formerly considered to be essential to emotional activity. In view also of the marked differences of the stimuli which are effective in each case, and the absence of 'after-discharge' in the decorticate preparation, it is evident that the processes of normal and decorticate emotions cannot be equated. Fear behavior has been demonstrated by Bard in the decorticate cat, but only with auditory stimuli. An essential problem remains in understanding cortico-subcortical interaction and the important role of perception in the fear responses of the normal animal.

The evidence presented has shown that the chimpanzee's fear of a detached head is in some way related to the physical lack of an attached body or of movement, or both. But our real interest is not in the physical properties of the stimulus object but in the way they act on the organism. The first question to be asked concerns the existence of a sensory control of the response: can one find any property of the sensory excitation which in itself determines the occurrence and form of the response?

The answer seems to be no. In the first place, the physical lack in the stimulus object cannot be equated with a sensory lack by saying that the sight of a head without the normally associated sight of a body causes fear, for the statement would not be true. When a chimpanzee sees a man's head only, without movement and with the rest of the man's body out of sight behind a door, he is not afraid. There are certainly sensory cues which distinguish the two situations (*i.e.*, detached head,

vs. attached head with body hidden) but I have not been able to find any generalization that distinguishes the purely sensory³ event which causes fear from the one which does not. In the second place, it has been shown that the fears are spontaneous. If they were also sensorily determined, it would follow that there are innate connections from the sensory cells excited in seeing any chimpanzee or human head to the motor centers determining avoidance; or in a more *Gestalt* formulation, that the dynamic properties of every such sensory excitation have an innately selective action on those particular motor centers. It would follow further that this sensori-motor relationship is consistently inhibited and nonfunctional throughout the animal's lifetime, no matter how many times he sees a human or chimpanzee head, unless by chance the head has been cut off from its owner. The improbability of such ideas is evident. They seem to be a product of the assumption (quite reasonable in itself) that the form of a response is fully determined by the sensory event that precipitates it: since a physical lack in the stimulus object cannot excite receptor cells, the assumption means that the part of the stimulus object which is present is an adequate excitant of fear, and, since the whole object does not cause fear, that the part which is missing is normally an inhibitor or in some way prevents an innately determined response to the other part. Such reasoning will be found to lead rapidly to absurdities. Doubt is then cast on the original assumption, and the alternative conclusion is indicated that

³ 'Sensory' in the present discussion is defined as referring to activity, in afferent structures, which is directly determined by environmental events; roughly, activity in the receptor organ and afferent tracts, up to and including the corresponding sensory projection area.

the determinant of certain strongly marked anthropoid fears is not any property of the sensory excitation alone but may have to be sought in some interaction of sensory events with other cerebral processes.

This argument depends on the accuracy of the analysis which has been made of the stimulating conditions in which fear of dismembered or inert bodies is observed. Other interpretations are possible, but seem either to beg the question or to amount to the same thing. (One might say, for example, that it is strangeness or mysteriousness that produces fear of a decapitated head and of an inert chimpanzee being carried by human beings. Actually, reference to strangeness only strengthens the preceding argument, as we shall see in a moment.) Nevertheless, it would be unwise to depend too strongly on the evidence of behavior into which so many complicating factors of experience may enter. Let us turn to fear of strangers (14) and of sudden noise (8). The theoretical interpretation suggested by fear of a dismembered body gains decisive support from these other observations and in turn makes their theoretical significance clearer.

The growing chimpanzee is persistently afraid of strange persons, objects and places, although the response is not always predictable in the individual case. Hebb and Riesen (14) have shown that the fear of strangers by chimpanzee infants is spontaneous and cannot be accounted for as a conditioned response. Also, a slight change of clothing may produce fear of a familiar attendant who was not feared before. To assume that the form of the response on seeing something strange is controlled alone by some property of the sensory event is to assume that *any* visual excitation is primarily a cause of fear and that other responses are substi-

tuted merely by repetition of the stimulation. Fear of strangers would mean that the visual excitation from any human or chimpanzee face (strange chimpanzees are feared as much as strange men) or any pattern of clothing is an innately adequate excitant of fear; for any pattern whatever may be strange, depending on accidents of experience. The idea seems absurd in itself, and is definitely contradicted by observations of the behavior of an infant chimpanzee blindfolded from birth to the age of four months, when the avoidance of strangers by normal animals is beginning (Nissen⁴). In Senden's (35) comprehensive review of the literature on persons born blind and given their sight after infancy, there is no mention of fear aroused by the first visual form-perception; and Dennis (7) explicitly denies that fear occurs in these persons. Fear of a strange person is therefore not determined by a particular property of the sensory excitation, but by some discrepancy of the pattern from those which have been frequently experienced by the subject—by a complex relationship, that is, of the sensory event to pre-existent cerebral processes.

A similar meaning lies in the fact noted by English (8) that a noise must be sudden to cause fear. When auditory intensity is built up gradually, the response is hard to elicit. The same is true of loss of support. An unexpected drop is the one that causes fear, not one for which preparation has been made verbally or by playful swinging of infant subjects. Jones (21) has shown that unexpectedness is an essential feature of a number of fear-provoking situations. In all such fears the major

determinant cannot be the afferent excitation alone but involves a relationship of that excitation to concurrent cerebral activity.

These facts actually raise no new theoretical issue. Their effect is to sharpen the definition of a problem which has been formulated in various ways by other writers. That both sensory and central processes are involved in the control of behavior and must be distinguished for theoretical purposes is implied by the concept of 'operants' (Skinner, 37) and of 'stimulus trace' (Hull, 16) no less than by the 'expectancy' of Cowles and Nissen (6), Mowrer (34) and Hilgard and Marquis (15). It is the real problem of attention and of the selectivity of response to the several properties of a sensory event (Leeper, 27; Lashley, 26). The problem is made explicit by Hilgard and Marquis's "central process which seems relatively independent of afferent stimuli" (p. 275), Beach's (4) 'central excitatory mechanism,' and Morgan's (33) 'central motive state.' Every serious attempt in recent years to analyze the neural mechanisms of the more complex forms of behavior has found the need of distinguishing, as more or less independent factors, sensory and central states or processes; in other words, of denying that the direction of transmission of a sensory excitation is determined by the properties of that excitation alone, even when the stable changes of learning have been taken into account. This is thoroughly consistent also with modern electro-physiology. All parts of the brain are continuously active and there are reasons for believing that the activity may be self-maintaining, and even self-initiating (1, 18, 29, 30, 41). An afferent excitation does *not* arouse inactive tissue, but modifies an activity already in existence. The conclusion, therefore, that there are nonsensory factors in the de-

⁴ Personal communication from Dr. H. W. Nissen. The experiment was not an investigation of emotional behavior, and detailed records on this point were not kept. But it is known with certainty that there was no avoidance evoked by the chimpanzee's first visual perception of human beings.

termination of certain fears agrees with existing theory.

It must be added that the conclusion is not necessarily trivial. Current opinion recognizes the necessity of postulating central determinants of behavior but it has done so reluctantly, always with reference to a single, rather narrow aspect of behavioral theory, and apparently without recognizing how generally the necessity has actually cropped up in psychological analysis. The preceding discussion may do no more than suggest a change of emphasis, but the change is one which, as I shall try to show, has a considerable effect on theory. Besides drawing attention to facts of behavior which are usually forgotten, it reveals some order in the facts and makes possible a coherent hypothesis of the nature of fear.

DEVELOPMENT OF AN HYPOTHESIS

Avoidance of strangers provides a possible starting point for a theory of the nature of fear. An essential feature of the stimulating conditions is the divergence of the object avoided from a familiar group of objects, while still having enough of their properties to fall within the same class. It is a most important fact that the fear or shyness does not develop at first vision, as the already cited data of Nissen, Senden (35) and Dennis (7) show. Common experience indicates also that the fear is minimized or absent if the growing infant has always been exposed to sight of a large number of persons. It is therefore dependent on the fact that certain perceptions have become habitual, a limited number of central neural reactions to the sight of human beings having been established with great specificity by repeated experience. The idea that there are such habits of perception was developed by Gibson (9) and further supported by later studies of the effect of set upon percep-

tion (5, 27, 43). A number of facts relating to the development of intelligence, and its changes with advancing age, have the same import (11, pp. 286, 289). From this point of view, it might be proposed that fear occurs when an object is seen which is like familiar objects in enough respects to arouse habitual processes of perception, but in other respects arouses incompatible processes.

Such a treatment of the fear of strangers would amount to an interference, incongruity or conflict theory. It might subsume fear of mutilated bodies as well, by classifying them as strange objects, and could be extended to cover fears due to pain and sudden loud noise, which obviously tend to disrupt concurrent psychological processes. But farther than this such a conflict theory will not go. There might be some difficulty in applying it even to the fear of strange objects, when the strangeness is apparently due to incompleteness in a familiar object (as with the chimpanzee's fear of a detached head); and conflict cannot account for causes of fear such as darkness (39) in which a sensory deficit is the effective condition, or nutritional disturbance (38).

Moreover, a fundamental question would remain as to the meaning of 'conflict,' and why an incompatibility between two perceptions should produce the incoordinations of emotional behavior. This is the crucial question, and in trying to answer it I believe we can find the possibility of a more comprehensive hypothesis, according to which conflict is only one of several ways in which a true source of fear occurs. If two perceptual processes, which cannot coexist, cannot even alternate without producing gross disturbances of behavior (which is what the conflict notion implies), ordinary unemotional behavior must depend on an essential temporal integration in

cerebral processes, and fear may be a direct result of their disorganization. Let us ask what such ideas would involve.

It has already been seen that sensory and central processes contribute separately to the control of behavior. For convenience, let us designate the specific pattern of cellular activity throughout the thalamo-cortical system, at any one moment, as a 'phase.' Behavior is directly correlated with a phase sequence which is temporally organized (4), in part by the inherent properties of the system (the constitutional factor) and in part by the time relations of various afferent excitations in the past (the factor of experience). The spatial organization of each phase, the actual anatomical pattern of cells which are active at any moment, would be affected by the present afferent excitation also. Subjectively, the phase sequence would be identified with the train of thought and perception. Now each phase is determined by a neural interaction, between the preceding phase and the concurrent afferent excitations. Lorente de Nó's (29) discussion of the dynamics of neural action shows that two or more simultaneous neural events might reinforce each other's effects and contribute to a single, determinate pattern of subsequent cerebral activity; or on the contrary might be indeterminate, in the sense that slight changes of timing and intensity could lead to marked and sudden fluctuations of pattern. A phase sequence, that is, could be stable or unstable, and one can assume that vacillating, unpredictable and incoordinated behavior is the expression of unstable cerebral activity. Also, the effect of learning in general is to increase the predictability and coördination of behavior. The element of learning in emotional behavior will be discussed more specifically, but in the meantime we may speak of the cerebral processes

controlling predictable, coördinated behavior as 'organized,' and recognize the tendency of learning to establish and maintain cerebral organization.

Disorganization could occur in several ways, some of which may be called conflict. (1) A sensory event might disrupt the concurrent phase sequence. The event might be one whose facilitation has been integrated into other phase sequences, and the disruptive only because it is 'unexpected.' If so the disruption would be brief, another well-organized phase sequence would be promptly established, and one would speak of the subject as having only been 'startled.' The disruption would be brief but it would occur; a well-organized phase could not be set up instantaneously, independent of facilitation from the preceding phase. On the other hand, the sensory event might fail to set up another organized sequence, and so initiate a prolonged disturbance; or might like loud noise, and especially pain, tend persistently to break down cerebral organization. (2) Simultaneous sensory events might have facilitations which are enough unlike to make the following phase sequence unstable, even though each event separately might be capable of integration with the concurrent phase. Evidently (1) and (2) would be modes of conflict, one sensory-central, the other sensory-sensory.⁵ But disorganization might also result (3) from the absence of a usually present sensory process. Cerebral organization involves learning. If a sensory activity *A* has always been present during adaptation to a sensory

⁵ Logically, another category of 'central-central' conflict would be possible, which might have some meaning with regard to emotional disturbances and anxiety arising from a conflict of ideas or beliefs. Such a concept might be applied to fear of socialism or of Catholics and emotional disturbances due to such purely intellectual ideas as those of Galileo or Darwin.

event *B*, facilitation from *A* would necessarily affect the final pattern of cellular activities which constitutes the adaptation to *B*, and might be essential to it. If so, *B* in the absence of *A* could again produce behavioral disturbance (if *B* without *A* occurs often enough, however, another adaptation would be established). Finally, (4) metabolic and structural changes in the central nervous system could obviously be a source of disorganization, by changing the time relations between the activities of individual cells, apart from any unusual conflict or sensory deficiency.

Attention must now be turned to the way in which cerebral processes tend to maintain their organization, in order to round out the picture of fear behavior. Whatever else may be true of it, avoidance certainly averts or minimizes disruptive stimulation. When we distinguish between the disruption and the processes tending to avert it, and assume that the degree of disruption may vary, we obtain the valuable result of seeing how a single mechanism of fear could on different occasions produce perfectly coordinated flight, a less coordinated avoidance accompanied by trembling and so on, startle, or the paralysis of terror: When cerebral disruption is extreme, it might presumably prevent any coordinated action, even flight.

It seems evident that the so-to-speak homeostatic processes which maintain the dynamic equilibrium of unemotional behavior are to a great extent processes of learning, operating in either of two ways. On the one hand there is negative adaptation to strange objects, which implies that a sensory-central conflict may be banished by an effect of learning on the central organization alone. The sensory event remains the same, yet disturbance disappears. With still further exposure, the formerly strange

object may become not merely tolerated but 'liked' and 'pleasant,' which is to say that the originally disturbing sensory event now actively supports cerebral integration.

On the other hand, learning may contribute to this integration indirectly, by reinforcing a mode of behavior (avoidance) which minimizes or removes the disturbing sensation. The incoordinations of emotional behavior, its most characteristic feature, are unlearned; they are apt to be most marked on the first occasion on which they are aroused by any particular stimulus. But the coordinated element of the behavior tends to become more prominent on repetition of the stimulus and to increase, while the unlearned incoordination is decreasing. It thus appears that the coordinated avoidance which occurs in fear behavior of normal animals is mainly learned.

There is indeed a primitive innate avoidance (manifested *e.g.* in the flexion reflex of Sherrington's (36) spinal animals, and the cowering of Bard's (3) decerebrate cats), but the avoidance which operates most efficiently to maintain coordinated effector activity is acquired. In the normal mammal at least, simple avoidance appears as a conditioned response to cues which in the past have preceded a disruptive stimulation. When a disruptive event is sudden and without warning the response is never an uncomplicated avoidance, a smooth and economical cooperation of effector organs, but involves startle, trembling, sweating, vocalization and so on. The optimum toward which behavior tends, with repetition of such disturbances, is a response (to premonitory cues) which completely averts the disturbing sensory event. At this final stage of learning, avoidance is fully effective in maintaining integrated cerebral action, and no emotional component is left in the behavior.

Thus avoidance without fear occurs. In the avoidance that does involve fear the learning process is not complete or premonitory cues have not been available, and the belated avoidance appears side by side with the excess of effector activity that justifies the inference of cerebral disorganization.

The reciprocal relationship of learning to the disruption of integrated behavior is most simply illustrated by an adult's unemotional avoidance of a hot stove which as an infant he may once have feared. Another illustration is provided by observation of adult chimpanzees where the course of learning in a very unusual social situation could be followed from its beginning. The experimenter, disguised with a grotesque false face and a change of clothing, approached each animal's cage wearing heavy gloves and acted the part of a very aggressive individual, instead of the cautious role one ordinarily takes with the chimpanzee. The results suggested an interpretation similar to that of Bridges (cited by Jones, 21) who concluded that an infant's fear develops out of primitive undifferentiated excitement. The first response by a number of animals was a generalized excitement and marked autonomic activity. An animal might be 'friendly' and viciously aggressive, almost in the same breath, or show erection of hair and scream and yet solicit petting. Attack, flight and the friendly pattern alternated unpredictably. As the stimulus was repeated over a 5-weeks' period, the autonomic activity decreased and one or other of the various patterns dominated. Eventually each animal's behavior became predictable, but it appeared often to be a matter of chance whether the original disturbance would develop into fear, aggression or (less often) friendliness. When avoidance became dominant, the animal would move back out of reach while the experimenter was still distant,

with a marked decrease of the excessive effector activity. Learning was clearly involved. We shall also see that the possibility, suggested by this example, that the learning may take more than one form, has a bearing on the theoretical relation of fear to other emotional patterns.

The hypothesis implicitly developed in this discussion can now be made explicit. The immediate source of fear is a disruption of a coördination, principally acquired, in the timing of cellular activities in the cerebrum. The disruption may be due to conflict, sensory deficit or constitutional change. With disruption there at once occur processes tending to restore the integration of cerebral activities; in fear these include either liminal or subliminal (13) activation of processes determining avoidance. Optimally, avoidance tends toward completely averting the cerebral disruption, and at this stage avoidance without fear would be said to occur.

CLASSIFICATION OF SPECIFIC FEARS

The value and limitations of the hypothesis will be clearer if we next see how it would be related to specific causes of fear.

(1) *Fears due to 'conflict.'* Here may be included fears induced by pain, loud noise, dead or mutilated bodies, and strange persons and animals. Pain and loud noise appear to have a primitive disrupting action, not psychologically analyzable nor dependent on any previous experience. To this extent fear of such things is in a special class. It is also noteworthy that there is little adaptation to the repetition of pain and sudden intense noise except in very special conditions (28).

Fear of the strange and of dead and mutilated bodies is included under the heading of conflict on the assumption that strange objects arouse incompatible perceptual and intellectual processes.

If it should be concluded however that the effective condition is a perceptual deficit, fear of the strange should be included in the following category (2). Finally, fear of snakes and certain small mammals may belong either in this or the following category. Although some basis for including them in the present category might be proposed it would be much too speculative, and it is best to let such fears stand for the present as not fully accounted for.

(2) *Fears due to sensory deficit.* Loss of support, darkness and solitude, as causes of fear, have in common an absence of customary stimulation.

Proprioceptive and pressure stimulation due to maintained position in space is practically always present, and it is plausible to suppose that the afferent excitation from these sources would have an essential part in maintaining experientially organized, or habitual, modes of cerebral action. With loss of support, however, the proprioception accompanying maintenance of posture against gravity, and exteroception from the surfaces on which the body's weight rests, are decreased or abolished. Redistribution of blood pressure and changes of position of the viscera would no doubt also lead to positive stimulation, but it seems unlikely that this is an effective cause of fear, in the infant. In the adult of course such stimulation would have become conditioned by experience. (If it should be true that positive visceral stimulation is the main cause of fear in an infant dropped for the first time, the fear should be classed in the preceding category (1), as one of those aroused by an unaccustomed stimulation.)

Fears induced by darkness and solitude (39) do not occur with time relations such that the emotional excitation can be attributed to the positive visual activity of the 'off-effect.' The response appears to be a genuine reaction

to deficit (25), intelligible only on the assumption of the present discussion that a 'response' need have no direct sensory excitant. The violent attempts of the growing chimpanzee to avoid isolation, even in full daylight, seem to require a quite similar interpretation. Köhler (23) has shown that the effective condition here is the social deprivation, as such. Just as a few patterns of postural stimulation are a practically constant feature of the afferent influx to the brain, and visual stimulation during waking hours, so social perceptions are frequent (though intermittent) and might be expected to become an integral element in the organization of cerebral action patterns. It is important to note that this would be a function of experience, and that no fear of darkness or of being alone should be expected in the subject who has only infrequently experienced anything else. Such a subject would develop a different cerebral organization, in which the perceptions referred to would play no essential part. It is also implied that in early infancy neither darkness nor isolation would have any emotional effect; and that as psychological development continues the patterns of cerebral action might toward maturity become so stable as to be relatively independent of any particular set of perceptions. Some adults, or most adults in a particular culture, might have no fear of darkness and isolation.

(3) *Constitutional disturbances and maturation.* Spies *et al.* (38) have provided exceptionally clear evidence that the psychotic fears so frequently found in pellagra are, in some instances at least, directly due to nutritional disturbance (see also Jolliffe, 20). When the psychosis is acute (before irreversible neural damage has been done), fear of friends and relatives and of hallucinatory creatures may clear up dramatically upon administration of nico-

tinic acid. The patient regains insight rapidly, can recall his fears clearly, but also is puzzled at the lack of any incident that might have caused them. Controls made to exactly define the action of nicotinic acid rule out psychological influence as essential either to the mental illness or its cure. Such fears must be regarded as originating in a disturbance of the metabolism of the individual cell, changing (for example) the timing of its detonation cycle and thus its relationship to the activity of other cells. In other words, metabolic changes would have disrupted the orderly sequence of cerebral events which is postulated here as the basis of normal unemotional behavior.

It is also evident that endocrine factors might at times produce a similar effect, partly accounting for the increased shyness and emotional instability of adolescence. The gonadal hormones must be supposed to have a selective action on certain central neural cells (4, 25) changing their properties of excitability and thus disrupting to a greater or less degree the neural organizations into which those cells have entered. With the passage of time reorganization would occur and shyness would decrease.

I do not of course suggest that constitutional changes are the only cause of shyness, or even the main cause. In its most pronounced form it must be thought of simply as an avoidance of strangers; and the next most important factor, after the sight of a strange person, may well be the fact that as the child matures others begin to behave toward him in a different way, according to his age. The child is confronted by 'strange' behavior, and situations which are strange to him. Thus shyness can be treated mainly as avoidance of the strange. It is not impossible however that structural and endocrine changes may also play a part in the

emotional instabilities of youth. One thinks of maturation as slow and gradual; but there is actually little evidence on this point, and spurts of growth might well make a significant modification of cerebral organizations established by earlier experience. In general terms, such an approach makes intelligible the sporadic appearance of the 'imaginative, subjective or anticipatory fears' classified as such by Jersild and Holmes (19). The fears referred to by Jersild and Holmes are markedly subject to maturation during a period of rapid and irregular growth, and when one observes them in the growing child it is characteristically hard to discover any sufficient cause in experience.

THE RELATIONSHIP OF FEAR TO RAGE AND OTHER STATES

Fear and rage are notoriously related, and it is impossible to frame any statement of the causes of rage (12) which would not on some points comprise causes of fear as well. The question, whether there is in fact any definite distinction, has been raised elsewhere (13). The hypothesis developed here suggests a kinship between the two emotions which may be put as follows:

The fundamental source of either emotion is of the same kind, a disruption of coördinated cerebral activity. Flight and aggression are two different modes of reaction tending to restore the dynamic equilibrium, or stability, of cerebral processes. The question may be left open at present whether there are different kinds of disturbance, one kind leading to rage, the other to fear. It seems almost certain that such a difference exists between extremes, but with no clear dichotomy; for in some situations, as I have suggested above, it appears to be a matter of chance whether aggression or flight will dominate behavior. Each of these modes of

response tends to restore integrated cerebral action, one by modifying the disturbing source of stimulation, the other by removing the animal from it.

Fawning would be another mode of reaction which would tend to modify disruptive stimulation (by placating the source). It is evident also that the hypothesis of this paper opens a wide field of speculation concerning a number of socially and clinically familiar conditions, such as shame, grief, chronic depression and so on. To deal with these varied emotional disturbances, the first step would be to classify the source of disturbance as modifiable by the subject's responses, or unmodifiable; and to further classify the modifiable according to the mode of overt reaction which would be effective. Thus shame or grief would arise from unmodifiable conditions; fear primarily from situations which are (or appear to the subject to be) modifiable by retreat; and so on. Finally, neurosis and some forms of psychosis would be regarded as a chronic condition of cerebral disorganization which according to the hypothesis might be initiated either by severe and prolonged conflict, or by a metabolic disturbance.

It would be idle at present to carry speculation farther, but it has been worthwhile observing that a theoretical relationship of fear to other emotional patterns is provided. If the proposed hypothesis is on the right track, the details of the relationship will become evident when more is known of the physiology of the cerebrum.

CONCLUSIONS

The conclusions of this paper may be put as follows:

(1) Anthropoid fears of inert, mutilated or dismembered bodies are spontaneous: that is to say, although experience of a certain kind is a prerequisite and learning is definitely involved, the

avoidance of such objects is not built up by association with a more primitive cause of fear.

(2) These and a number of other fears are evidently not determined by a sensory event alone, and the behavior is not intelligible except on the assumption that its control is a joint product of sensory and 'autonomous' central processes. Consequently no amount of analysis of the stimulating conditions alone can be expected to elucidate the nature of fear, or to lead to any useful generalization concerning its causes.

(3) An adequate hypothesis of the nature of fear cannot be framed in psychological terms alone, but must utilize physiological concepts of cerebral action. No common psychological ground can be discovered for all the various causes of fear. What is there in common, for example, between the characteristically high level of the auditory and low level of visual stimulation which induces fear in children? or between fear of strangers, which decreases, and fear induced by pain, which tends to increase, with repetition?

The hypothesis developed here has made a considerable synthesis of formerly unrelated facts, although it remains vague on some crucial points. It proposes in brief that fear originates in the disruption of temporally and spatially organized cerebral activities; that fear is distinct from other emotions by the nature of the processes tending to restore cerebral equilibrium (that is, *via* flight); and classifies the sources of fear as involving (1) conflict, (2) sensory deficit or (3) constitutional change. By distinguishing between processes which break down and those which restore physiological organization in the cerebrum, the variability of fear behavior is accounted for.

The conceptions of neurophysiological action on which this is based were developed originally as an approach to

other problems, and will be presented in detail elsewhere. When this is done, and the neurophysiological implications are made explicit, it may appear that a basis has been laid at last for an adequate theory of emotion and motivation—something which is lacking in psychology at present.

REFERENCES

- ADRIAN, E. D. Electrical activity of the nervous system. *Arch. Neurol. Psychiatr.*, 1934, 32, 1125-1136.
- BARD, P. On emotional expression after decortication with some remarks on certain theoretical views. *Psychol. Rev.*, 1934, 41, 309-329.
- . Neural mechanisms in emotional and sexual behavior. *Psychosom. Med.*, 1942, 4, 171-172.
- BEACH, F. A. Analysis of factors involved in the arousal, maintenance and manifestation of sexual excitement in male animals. *Psychosom. Med.*, 1942, 4, 173-198.
- CARMICHAEL, L., HOGAN, H. P., & WALTER, A. A. An experimental study of the effect of language on the reproduction of visually perceived form. *J. exp. Psychol.*, 1932, 15, 73-86.
- COWLES, J. T., & NISSEN, H. W. Reward-expectancy in delayed responses of chimpanzees. *J. comp. Psychol.*, 1937, 24, 345-358.
- DENNIS, W. Congenital cataract and unlearned behavior. *J. genet. Psychol.*, 1934, 44, 340-350.
- ENGLISH, H. B. Three cases of the 'conditioned fear response.' *J. abnorm. soc. Psychol.*, 1929, 24, 221-225.
- GIBSON, J. J. The reproduction of visually perceived forms. *J. exp. Psychol.*, 1929, 12, 1-39.
- HASLERUD, G. M. The effect of movement of stimulus objects upon avoidance reactions in chimpanzees. *J. comp. Psychol.*, 1938, 25, 507-528.
- HEBB, D. O. The effect of early and late brain injury on test scores, and the nature of normal adult intelligence. *Proc. Amer. phil. Soc.*, 1942, 85, 275-292.
- . The forms and conditions of chimpanzee anger. *Bull. Canad. Psychol. Assoc.*, 1945, 5, 32-35.
- . Emotion in man and animal: An analysis of the intuitive processes of recognition. *Psychol. Rev.*, 1946, 53, 88-106.
- , & RIESEN, A. H. The genesis of irrational fears. *Bull. Canad. Psychol. Assoc.*, 1943, 3, 49-50.
- HILGARD, E. R., & MARQUIS, D. G. *Conditioning and learning*. New York: Appleton-Century, 1940.
- HULL, C. L. *Principles of behavior: An introduction to behavior theory*. New York: Appleton-Century, 1943.
- JACOBSEN, C. F., JACOBSEN, M. M., & YOSHIOKA, J. G. Development of an infant chimpanzee during her first year. *Comp. Psychol. Monog.*, 1932, 9, 1-94.
- JASPER, H. H. Electrical signs of cortical activity. *Psychol. Bull.*, 1937, 34, 411-481.
- JERSILD, A. T., & HOLMES, F. B. *Children's fears*. New York: Teachers College Bureau of Publications, 1935.
- JOLLIFFE, N. The neuropsychiatric manifestations of vitamin deficiencies. *J. Mt. Sinai Hosp.*, 1942, 8, 658-667.
- JONES, M. C. Emotional development. In *A handbook of child psychology*, 2nd ed. (C. Murchison, Ed.). Worcester, Mass.: Clark Univ. Press, 1933, 271-302.
- JONES, H. E., & JONES, M. C. A study of fear. *Childhood Educ.*, 1928, 5, 136-143.
- KÖHLER, W. *The mentality of apes*. New York: Harcourt, Brace, 1925.
- LASHLEY, K. S. The thalamus and emotion. *Psychol. Rev.*, 1938, 45, 42-61.
- . Experimental analysis of instinctive behavior. *Psychol. Rev.*, 1938, 45, 445-471.
- . An examination of the 'continuity theory' as applied to discrimination learning. *J. gen. Psychol.*, 1942, 26, 241-265.
- LEEPER, R. A study of a neglected portion of the field of learning. The development of sensory organization. *J. genet. Psychol.*, 1935, 46, 41-75.
- LIDDELL, H. S. Animal behavior studies bearing on the problem of pain. *Psychosom. Med.*, 1944, 6, 261-263.
- LORENTE DE NÓ, R. Transmission of impulses through cranial motor nuclei. *J. Neurophysiol.*, 1939, 2, 402-464.
- . Cerebral cortex: architecture. In *Physiology of the nervous system*, 2nd ed. (J. F. Fulton, Ed.). New York: Oxford Univ. Press, 1943, 274-301.
- MCCULLOCH, T. L., & HASLERUD, G. M. Affective responses of an infant chimpanzee reared in isolation from its kind. *J. comp. Psychol.*, 1939, 28, 437-445.

32. MASSERMAN, J. H. The hypothalamus in psychiatry. *Amer. J. Psychiatr.*, 1942, 98, 633-637.
33. MORGAN, C. T. *Physiological psychology*. New York: McGraw-Hill, 1943.
34. MOWRER, O. H. Preparatory set (expectancy)—a determinant in motivation and learning. *Psychol. Rev.*, 1938, 45, 62-91.
35. SENDEN, M. v. *Raum- und Gestaltaufassung bei operierten Blindgeborenen vor und nach der Operation*. Leipzig: Barth, 1932.
36. SHERRINGTON, C. S. *Integrative action of the nervous system*. New York: Scribner's, 1906.
37. SKINNER, B. F. *The behavior of organisms: An experimental analysis*. New York: Appleton-Century, 1938.
38. SPIES, T. D., ARING, C. D., GELPERIN, J., & BEAN, W. B. The mental symptoms of pellagra. Their relief with nicotinic acid. *Amer. J. med. Sci.*, 1938, 196, 461-475.
39. VALENTINE, C. W. The innate bases of fear. *J. genet. Psychol.*, 1930, 37, 394-419.
40. WATSON, J. B. *Behaviorism*. New York: Norton, 1924.
41. WEISS, P. Autonomous versus reflexogenous activity of the central nervous system. *Proc. Amer. phil. Soc.*, 1941, 84, 53-64.
42. YERKES, R. M., & YERKES, A. W. Nature and conditions of avoidance (fear) response in chimpanzee. *J. comp. Psychol.*, 1936, 21, 53-66.
43. ZANGWILL, O. L. A study of the significance of attitude in recognition. *Brit. J. Psychol.*, 1937, 28, 12-17.

EXPECTATION

BY E. L. THORNDIKE

What has come to be known as S-R psychology has paid little attention to expectations (or expectancies, as Tolman now prefers to call them). For example, Hull's recent book (2) says nothing whatever about them, if my reading and memory can be trusted. My own *Fundamentals of Learning* (4) merely quoted statements of Tolman (6) and Hollingworth (1) in which 'representations of the ends of acts' were assumed to strengthen propensities and to make the law of effect superfluous, admitted the frequent occurrence of such representations, but argued that they did not make the law of effect superfluous. In *The Psychology of Wants, Interests, and Attitudes* (5) I did include a sketchy account of expectations, but limited their influence by this drastic and cavalier sentence, "Whatever be the detailed nature of his (i.e. an animal's) expectation, its potency will depend on its connections" (5, p. 85).

With this sentence we may contrast Tolman's recent reaffirmation that "An intelligence functioning (that is, a success functioning in the reaching of a goal) is, as I see it, an expectancy on the part of the organism, aroused by that part of the stimulus lay-out which is immediately presented, to the effect that such and such performances or behaviors (if carried out) would be successful in reaching such and such a goal"¹ (9, p. 164).

¹ Directly after this Tolman says, "These expectancies fundamentally are merely sets in the nervous system aroused by environmental stimuli." If they are merely that, they should have no *intrinsic* powers beyond what any environmentally caused sets in the nervous system have. If they are merely that, there seems no reason to call them sign-Gestalten, or to contrast learning that involves them

What is the truth of the matter? Since, according to Tolman, expectancies need not be, or be accompanied by, any conscious ideas, images, hopes, fears, impulses, or the like, I shall keep to cases within that limitation as long as I can.

When an animal responds repeatedly to a certain situation by a certain act, there may intervene between the situation and the act something reasonably called an expectation. This something may be clear and emphatic as when a reader moving his line of vision back from the end of line 1 to the beginning of line 2 of a page 'expects' to see words, words different from those just read, words that make sense when added to those just read. It may be dubious and faint as when a reader expects to be interested by lines 2, 3, 4, 5, etc. but instead gradually becomes bored. The amount of such expectations associated with a process of learning may approach zero as a limit. For example, in playing tennis and improving thereat, almost no expectation may intervene between any of the external situations and the player's movements.

Rats running mazes or choosing in discrimination experiments may show a considerable amount of what I have elsewhere called an 'embryonic sort of expectation.' For example, they are disturbed if the food-box lacks contents or has different contents. In forming habits that get them quickly to the goal in a certain maze they also

with learning by occurrences and after-effects. So I have assumed that the 'merely' was inadvertent, and that Tolman's real intent was to emphasize the fact that, as he says later, "such neurally-based expectancies are . . . often accompanied by consciousness; but they need not be."

learn more or less about the general topography of that maze, and this so-called 'latent' learning may reasonably be supposed to include 'expectations' at various points of what will come next if a certain choice is made.

We may all agree that if such an expectation is responded to by a certain act one or more times the probability that it will be at its next occurrence may be increased, especially if the sequence of expectation \rightarrow act has a satisfying after-effect. Does an expectation also have an intrinsic power in learning apart from what it has led to and with what consequences? Tolman (6, 7, 8, 9) has long maintained that it can and often does. Are Tolman's 'sign-Gestalt-expectations' like hunger, thirst, startle, itch, and nausea in having underived powers to evoke certain acts, or are they like the great majority of words, which get all their power from the meanings that have been associated with them?

The most favored cases for the former view are that an expectation of finding food down a certain alley will in and of itself move a hungry animal down that alley, that an expectation of finding water behind a white card will in and of itself move a thirsty animal to jump through the hole in that card, etc. The former view is very plausible for these cases; but nothing in the experiments with rat learning *proves* it even for them. That a rat impelled by its training to go down a certain alley to get food will, when prevented from normal running down said alley, climb down it by the screening at its top does not prove it, since the power that switches him to climbing may reside, not in his expectation of food, but in his impulse or mental set to get to the end of that alley. That a rat on finding the previously satisfying object lacking in the goal should more or less suddenly abandon the habit of promptly proceed-

ing toward that goal does not prove it. The effective alteration may be in the rat's impulses or mental set rather than its expectations. So-called 'latent learning' does not prove it, since the expectations acquired in 'latent learning' have many connections and these may be essential to the potency of the expectations.

What is needed for crucial experiments on the intrinsic power of expectations is, of course, to arrange matters so as to measure the power of any expectation barren of acquired associations, and uncontaminated by forces other than expectations. I suggest the following experiments which aim to provide opportunities to acquire expectations with a minimum of helpful associations or accompanying impulses:

A. Put the rat, in a little wire car, in the entrance chamber of a maze, run it through the correct path of a simple maze and into the food compartment. Release it there and let it eat the morsel provided. Repeat 10 to 100 times according to the difficulty of the maze under ordinary conditions. The rat has had an opportunity to form expectancies that presence in the food compartment is followed by food, that the last correct turn is followed by presence in the food chamber, and so on. Then put it in the entrance chamber free to go wherever it is inclined and observe what it does. Compare the behavior of such rats with that of rats run in the customary manner.

B. Have a pen with a platform say 6 in. square and 2 feet high led up to by a ladder. Take a rat, hold it gently in one corner of the pen, move it slowly to the foot of the ladder, up parallel to the ladder, and on to the platform. Release it there and give it a choice morsel of food. Repeat ten times a day for five days. Then put it in the corner of the pen to do whatever it is inclined to do and observe what it does. The rat

has had an opportunity to form expectancies that presence on the platform meets with food and that translation in space from the corner to the foot of the ladder and upward in the direction of the ladder gives quick access to the platform. Compare the behavior of such rats with that of rats put in a corner of such a pen, left free to explore it, and given a similar morsel when they reach the platform.

C. Have a pen with a food box that is solid except for an opening at one side covered by a sheet of tissue paper and a small funnel in the top through which food can be dropped. Take a rat, hold it gently in one corner of the pen, move it to a position in front of the paper, hold it there with one hand and with the other take its front legs and with them rip down the tissue paper. Push the rat into the food box and drop the morsel of food down the funnel. Repeat ten times a day for two days and test. If the result is negative repeat twenty times more and test. Compare the results with rats that have had no opportunities to acquire expectancies about the food box.

D. An experiment like C, but with digging away sawdust to uncover an opening into the food box. Experiments C and D differ from A and B in that more than locational and directional tendencies are required to be strengthened. It is possible that expectations of scratching and digging might be harder for rats to form or less potent when formed than expectations about places and directions.

I do not claim that the expectations in these experiments will be identical with those which would develop in ordinary trial, error, and success learning with the same mazes and pens, but only that they will be as nearly identical as they can be made without giving opportunity for the formation, by repetition and reward, of direct associations

between situation and act, or between expectations and acts.

If the animals learn the solution of a problem as well from a score of these opportunities (of experiments A, B, C and D) to acquire an expectation that such and such behavior will be 'to the good' as from a score of comparable active and successful quests, that fact would be strong evidence that expectations have power in and of themselves. If they make no appreciable progress toward the solution of a problem by such opportunities, it seems probable that an expectation gets power by its associations rather than by its intrinsic nature.

There is another set of relevant observations to the effect that, where conscious expectations do appear, learning may yet progress to a substantial amount before they appear. Let human subjects be told that the 'right' response to each of a set of say 40 words is some number from 1 to 5, be required to say a number in response to each word as the set is read (in varying orders), and be told 'right' or 'wrong' after each response. Their percentage of correct responses rises from 20 by substantial amounts before the hearings arouse any appreciable number of conscious expectations of anything save that the experimenter will say more words. The subject continues to guess blindly but his guesses are more often right.

When expectations do appear they do not, in my experience, precede the act of saying the 'one' or 'two,' etc. much oftener than they follow it. They still oftener consist in a mere feeling of confidence accompanying the act. Judging from my own experience, it is more probable that strong tendencies to say 3 for *bolt*, 2 for *play*, and 5 for *master* caused the expectations 'bolt-3 is right,' 'play-2 is right' and 'master-5 is right,' than that these expectations caused the

tendencies to say 'three,' 'two,' etc. A repetition of such experiments with entire freedom of the subjects to wait as long as they liked before making a choice might be useful.

Experiments on cats and dogs somewhat comparable to those suggested above for rats showed no evidence of intrinsic power of expectations. For example, a cat is induced to go into a puzzle-box through the doorway, which is then closed. It pulls a loop that opens the door, goes out, and is given a bit of fish. After enough occurrences of this (17, 25, and 36 for the three cats used) the cat will frequently go into the box of its own accord as soon as it gets the fish. If, however, a cat is *dropped* into the puzzle box through a hole in its top instead of being induced to *go* in, all else in the experiment being the same, it will not go into the box of its own accord (after 50, 60, and 75 occurrences, in the three cats so treated). [Thorndike, 1898, p. 67 f.]

The kittens who were dropped into the box from the top had nearly or quite as good opportunities to 'expect' that being in the box was a preliminary to getting out of it and obtaining food as the kittens who went back in through the doorway.²

Many psychologists who favor a moderate connectionism and see no advantage in substituting expectations for habits or in substituting closure, equilibrium, *et al.* for repetition and reward, will still be shocked by the assertion that an expectation has no intrinsic

internal power by its very nature. They will think such an assertion is contradicted by countless phenomena of suggestion, especially in hypnotized subjects and abnormal minds. They may be right. Why otherwise should a hypnotized subject smack his lips after a nauseous drink that he expected to taste sweet, or laugh heartily at a series of nonsense syllables which he expects will be a joke, and weep at the same series if he expects it to be a sad tale?

I can make only the weak answer that if all the habitual connections leading from the drink and the nonsense syllables, are shut off, leaving the hypnotizer's, "Here is a superb wine." "Listen to this joke," etc. to evoke what has gone with them or situations like them, the subject would by the force of past occurrences and after-effects make responses more or less of the sort described. This answer, though weak, may be true. The results of Hull's experiments on suggestion (2) are in its favor; in its favor also is the fact that the individual differences in what a suggestion or expectation does in a hundred persons all inoculated with as nearly as possible the same suggestion or expectation seem attributable to differences in the associations it has. If the expectation of a pleasing taste makes different persons respond to the nauseous drink by smacking the lips, saying 'Fine,' saying 'yum-yum,' uttering a hearty 'Ah,' or rubbing the stomach in accordance with their peculiar associations, the general consequence of the expectation is presumably due to its general associations.

Let us turn now to some less controversial matters. First let us compare behavior where expectation is prominent or at least present, with behavior where it is absent or at least not easily demonstrated. Call these for short *E* and *Non E*. When your cat

² I take this occasion to make a criticism of the Thorndike experiments of 1898 which I have postponed for forty years because of a curiosity to see how long it would be before somebody did make it! It is that most of the animals were not adult, and so not fair samples of cat and dog mentality, particularly with respect to the infrequency of learning by ideas. The criticism is valid but probably not important. Old cats may well have more ideas than kittens four months old, but no different procedures in using them.

hears you call 'Kitty, Kitty,' sees and smells the fish that you hold, and follows you as you walk to her food dish, her behavior is *E*. When she then laps the milk but not the fish, bites the fish but not the milk, neglects the bits of scrambled egg and bread until the fish is eaten, and neglects the bread until the egg is eaten, her behavior is *Non E*. When you want a drink of water, get a glass, and hold it under the tap until it is full, you manifest more or less of *E* behavior, but from the time you put the glass to your lips until you have drunk your fill, *Non E* habits act with few or no expectations of any sort. Speaking roughly, expectation in the ordinary use of the word is very frequent in deliberation and invention, fairly frequent in dealings with information, relatively infrequent in direct dealings with objects.

It seems to me that the useful functions of expectations are the same as some functions of knowledge, that whatever an expectation can do in adapting behavior to things, qualities, events and relations knowledge can do as well or better, that knowledge often operates via expectations—but it may operate otherwise—and that Tolman's claims would be stronger if made for knowledge instead of for 'sign-Gestalt expectations.' It seems to me, therefore, that what $S \rightarrow R$ psychology has to learn from Tolman's work is the need for a satisfactory account of primitive forms of knowledge, and of how they operate.

I could describe a long program of observations and experiments for this purpose, but will limit myself to two suggestions. First, we should explore the receptive or perceptive behavior of animals and the linkages between its simultaneous and successive elements with the same enterprise that has been devoted to motor behavior and linkages between an event happening to an animal and an act performed by him. Knowledge can come from both sorts, but knowledge coming from the former

is, so to speak, purer, as it is less confused by elements of skill and conduct. Second, we should, in experiments to search out the primitive forms of knowledge, preserve a constant order of nature, never changing the contents of any compartment, nor the act or acts necessary to gain access to it, nor the meaning of any signal given by the experimenter. This will reduce frustrations of knowledges and conflicts between knowledges, and so encourage their retention if they appear.

I do not anticipate that the study of primitive forms of knowledge will reveal any gross errors in the connectionist explanation of learning, but it may remedy some inadequacies and point out some extravagances. Among the latter will probably be my cavalier sentence of 1935, which I now amend to read: "Whatever be the nature of an animal's expectation, its potency will depend mainly, perhaps entirely, on its connections." What amendments Tolman will need to make in his statements is not for me to say.

REFERENCES

1. HOLLINGWORTH, H. L. *Psychology; its facts and principles*. New York: Appleton-Century, 1928.
2. HULL, C. L. *Hypnosis and suggestibility*. New York: Appleton-Century, 1933.
3. ——. *Principles of behavior, an introduction to behavior theory*. New York: Appleton-Century, 1943.
4. THORNDIKE, E. L. *Fundamentals of learning*. New York: Bureau of Publications, Teachers College, 1932.
5. ——. *Psychology of wants, interests, and attitudes*. New York: Bureau of Publications, Teachers College, 1935.
6. TOLMAN, E. C. Habit formation and higher mental processes in animals. *Psychol. Bull.*, 1928, 25, 24-53.
7. ——. *Purposive behavior in animals and men*. New York: Appleton-Century, 1932.
8. ——. The determiners of behavior at a choice point. *PSYCHOL. REV.*, 1938, 45, 1-41.
9. ——. A stimulus-expectancy need-cathexis psychology. *Science*, 1945, 101, 160-166.

THE GESTALT VIEW OF THE PROCESS OF INSTITUTIONAL TRANSFORMATION

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THE RISKINESS OF THIS UNDERTAKING

The comprehensive interpretation of natural phenomena which is characterized by the kindred names of 'Gestalt' in psychology, 'field' theory in astronomy and physics, and 'organismic' biology in the life sciences did not originate in any direct contemplation of the social scene. The broader application of concepts derived from these highly academic and specialized constructions runs the risk of being repudiated by those who consider it an unwarranted extension of technical ideas to a domain where they are said to lose their proper meaning and to lack all 'fruitfulness.' In addition, such an attempt raises all the methodological dangers of analogical thought such as typified by David Hume's description of the associative bond as a kind of 'mental gravitation.'

Such objections are legitimate enough, provided they foster caution and self-criticism and stop short of a crippling inhibition upon the exercise of the theoretical imagination. From a purely formal point of view, *e.g.*, it is clear that the problem of the relations of the individual and the group is but a special case of the universal Gestalt problem of the part and the whole, perhaps the most primitive differentiation of which the human mind is capable. Social aggregations exhibit the properties of 'weak' *Gestalten*, *i.e.*, structural entities which reveal considerable local autonomy as contrasted with 'strong' *Gestalten* like an electrical circuit or a soap-bubble where the reciprocal or unitary interaction of the components is more marked. It may be that these

resemblances of organization are purely metaphorical, but the social psychologist may derive some comfort and support for his speculations from the precedent of the physicists who, for pedagogical reasons, have occasionally envisaged the interior of the atom as a miniature solar system.¹

SOME BASIC GESTALT PRINCIPLES

The essence of all field-theoretical modes of thought is found in the postulate (usually considered a universally valid generalization) that every phenomenon is a result or 'function' of the special pattern of forces operative in the finite spatio-temporal field wherein it occurs. Otherwise stated, all observable events, objects, or qualities in nature are products of the conditions that create and sustain them. The most important characteristic of these conditions is the way in which they are organized. The things we perceive are a consequence of the arrangement of forces that evoke them—change this structure and the data of perception change too.

This is the substance of what might be called the all-embracing Gestalt 'physics' or cosmology, of which the

¹ See D. L. Watson (3, Chap. 6). This discussion is built entirely around the latitudinarian Gestalt concept of *isomorphism* which is presented as a basis for the liberalization and poetizing of scientific thought. Intellectual 'respectability' is conferred by an approving foreword from John Dewey. Another highly relevant reference, indicative of the effort to save 'frontier analogies' from scornful rejection by more classically-minded colleagues, is Julian Huxley's 'Analogies: dangerous and otherwise' (2).

psychology or sociology is really a special instance. The notion of *system* or interdependence in one form or other is central. The behavior of the earth is dependent upon the frame of the total solar system of which it is a part; bodily exertion is harder for man three miles above sea level than at sea level itself; a toothless person does not speak the same as one with his front teeth intact; the word 'cast' has a different meaning in a 'hardware' context than in a fishing or theatrical situation. The common factor in all these apparently disparate instances is that the setting of an act is decisive for the act and for all or most of its attributes.

Those who have familiarized themselves with the many psychological phenomena which are illuminated by the organismic approach to human behavior find it hard to escape the recurrent, if somewhat vague, impression that societal happenings are also made more intelligible through the insights it affords. Indeed, the laws that determine *social* behavior are not just *like* those that govern *individual* reactions—they are the *same* laws. The accidents of history brought a Gestalt psychology into being before a Gestalt sociology (or Gestalt economics, anthropology, politics, etc.) developed. From a classificatory point of view, however, the configurations discerned by the social sciences are, if anything, more 'fundamental' than those isolated by the laboratory psychologist, in the sense that they constitute larger and more inclusive realities.

AN APPRAISAL OF BROWN'S PIONEERING VENTURE

The only major attempt to utilize field theory in interpreting social change is J. F. Brown's *Psychology and the Social Order* (1). This volume makes extensive use of Lewin's system of topological portrayal with an originality that

is both brilliant and profound. He rightly shows that there is diminished social locomotion for all groups in times of economic crisis, but particularly for the 'proletariat,' a fact which normally increases tension and accentuates class conflicts.

During times of business prosperity—or war against an external enemy—the barriers separating different social categories become more permeable. The potency of membership-character in a group can be raised or lowered by changes in the culture external thereto. If one thinks of the individual as comparable to Rubin's 'figure' and the community or its active subdivision as equivalent to the perceptual 'ground,' much is gained by recognizing that an alteration in the properties of the ground necessarily changes the properties of the figure. Brown's rich account of these dynamic interactions often displays great subtlety.

It must, however, be emphasized that Brown's discussion, while made with all the acute understanding of an ardent disciple, fails to conform with objective reality at a number of points, particularly in his unfavorable treatment of evolutionary socialism as contrasted with the glowing account of the Communist dictatorship. Events since 1936, the date of publication—if not the 'facts' even then fully available, but apparently less welcome during the Popular Front era when the book was developed—have shown all too plainly that 'field theory' *per se* is no royal road to the correct appraisal of the social scene.

A FEW WORDS ON THE VALUE PROBLEM

The lesson to be drawn from this grandiose error is not that field theory is irrelevant to social organization, but that *one must know reasonably intimately and completely the situation to*

which it is being applied. Otherwise, political naiveté may lead to gross misinterpretations of events; the effect may be a popular discrediting of a legitimate enough body of generalized concepts when all that has really been discredited is a specific sectarian outlook and practice with which it has adventitiously been associated. Authors and readers may both be guilty of *non-sequiturs*. Field theory may help account for what has occurred, and is happening, in the United States, Germany, and Russia, and elsewhere; but the differential appraisal one bestows upon these various cultures is a function of one's entire value system and not of one's knowledge of Gestalt thinking *per se*.

To one who values honesty, the U. S. S. R. may seem a gross fraud; to one who valued kindness, Nazi Germany seemed to be dominated by cruel men; to one who values economy, America appears shockingly wasteful of human and natural resources. What this means is that the critic has one hierarchical scale of values and those in power in the respective countries another. Social conflicts occur because different groups have different effective values, with too few common values to unite them. The critic's values, too, have their own natural history. Values themselves are not outside the domain of social psychology—indeed, they lie at its very core, as they do at the heart of all human experience—but at present one cannot consider any particular set to be the only objectively valid order of worth the world contains. A scientist is entitled to his preferences as much as any one else, but we cannot without fuller demonstration view every expression of a scientist's preference as itself mandatory for others, whether scientists or not. The search for a universally valid, *i.e.*, world-wide, pattern of human and social values must continue; it probably exists, and a

deeper knowledge of the common demands imposed upon men by the very structure of their organisms will facilitate its discovery. Meanwhile we must employ approximations to that which we seek but have not yet found.

WERTHEIMER ON DEMOCRACY

Another attempt to apply configurational analysis to a 'controversial' social situation appears in a neglected paper by Max Wertheimer (4).² Although he does not say so, it seems fairly clear from the time and circumstances of the writing of this article that Wertheimer had in mind the contemporary conservative (chiefly Republican) attack upon the enlargement of the State's services in the interests of the entire community and the progressive's fairly constant endeavor to increase the sphere of economic activity embraced by public ownership. The attitudinal opposition here confronted is the older historic nineteenth century Jeffersonian or Spencerian liberalism which sought to reduce all governmental powers to the absolute minimum vs. the newer liberalism (= social democracy) which sees in a popularly-controlled government a powerful instrument for community advance.

Wertheimer holds that part of the difficulty is found in the methods by which the rival groups arrive at their notion of the constituents of the desideratum, democracy. The older procedure is to compare it with other forms of government, specifying similarities and differences. The newer method is to "investigate the structural function of the items, the hierarchical structure of the whole idea" (4, p. 272). This means that a distinction must be made between central and peripheral items. *The central idea of democracy is that a*

² All the contributors to this volume were members of the 'University in Exile' of the New School for Social Research.

just and reasonable society can be built by a well-informed population. This is, in essence, an elementary hypothesis of all formal or systematic education. Even the cherished autonomy of the individual (= civil liberty) is structurally secondary, an outcome of the first principle in a special direction. "The will to courageous truth, to objective reason, to just decisions, requires freedom of the man and of his mind, but this does not involve the harmony theory of *laissez faire* in enterprise" (4, p. 279).

Certain court decisions, Wertheimer declares, are reached by artificially narrowing the mental field, viewing parts as if they were in themselves the vital thing. American political democracy, however, is to be viewed *not* as a structure in itself but as a *part* in the social field, in the larger whole. If an important change—say monopoly capitalism—occurs in the social field in which democracy is functioning as a part, then stresses and strains are born and the 'system' has to face new problems. Disequilibrium in one part of the field, *e.g.*, the economic, is not simply irrelevant, because the total dynamic system is affected. Such new tensions give a different meaning to such an old item as 'freedom of contract.' These new realities must be taken into account in order to reestablish the democratic principle—and that requires an effort at improvement, reform, and reconstruction so that the system may work again consistently with the demands of the inner meaning of democracy, for the old forms now produce nothing but superficial freedom.

"Viewed in this way the real essence of democracy seems to be not a form of government, a sum of institutions, etc., but a certain real attitude in life, behavior of a certain kind, not only in state matters but generally in relations between men. This attitude has some characteristic similarities

to the role of the judge or the juror, rather than to the fighting of interests" (4, p. 280).

As a relativist position, Gestalt theory evidently supports the view that a society compatible with reason and justice may have one design in 1800 and 1900 and quite another in 1946 and 1970. The realization of a given social ideal does not mean a change in the ideal so much as a change in the instruments for its attainment. Liberals and socialists—or at least some of them—presumably possess this insight into the organic quality of the culture; typical 'old-fashioned' Republicans, *per contra* (with the author's profuse apologies for these crude stereotypes!), may be said to be thinking atomistically on social issues and therefore ineffectively and non-adaptively about the problems they face. This emphasis upon the *coherence* of the social scheme is not peculiar to the configurationists—it is at least as old as Karl Marx, if not Robert Owen or Rousseau—but they have shown dramatically that the reasons for this lie in the very nature of our thought and action processes, whereas earlier writers had to content themselves with more limited descriptions of the relations they discerned.

SOCIAL BEHAVIOR PATTERNS AND CLIMATE

The only other field theorist in psychology who has written at any length on its pertinence to the elusive question of social change is Raymond Wheeler. Perhaps his chief contribution in this area is his postulation and partial demonstration of the existence of a 'world climate.' Acting on the assumption that the available energy in man's environment is primarily responsible for the rises and falls in his own activity, Wheeler has calculated with prodigious industry the rainfall and

temperature correlations for different parts of the earth. From the fact that these correlations, while often low, are consistently positive between the various continents, he argues that the common factor must be a *general planetary climate* ultimately created by atmospheric disturbances originating in the sun. These energy fluctuations apparently obey a definite long-term cycle of the recurring sine wave type from cold-dry to warm-dry to warm-wet to cold-wet weathers or climates, followed by cold-dry which initiates the four-fold cycle again.

More significant than this meteorological curio as such is the claim that certain characteristic historical epochs can be ordered to each of the four main phases of the grand climatic cycle. Man's profoundest cultural achievements, his Golden Ages, are said to occur at the *axis* of the wave. Widespread international or civil wars are associated with a maximum of solar energy—presumably whenever human beings feel energetic due to heightened sun-spot formation they are disposed to engage in actions which eventually lead them to slay each other! Even the most delicate thought forms are influenced by this climatic cycle, for the classical-romantic, totalitarian-democratic, organic-mechanistic, and other ideological antinomies move in rhythm with the separate phases. A chronological chart covering thousands of years of recorded history and some pre-history seems to show that tree-ring growth in sequoias and petrified trees and data pertaining to changing lake levels are good indices of an oscillating 'world climate,' and that these changes are associated with variations in the amount and quality of human behavior.⁸

The usual initial reaction to this presentation is one of utter incredulity—it

⁸ The most convenient brief reference is an article by Wheeler (5).

seems like absolute determinism run wild. Nonetheless, geographers and ecologists are not disposed to dismiss it without further inquiry.⁴ It is a distinct advance upon the earlier work of Ellsworth Huntington, conceived in much the same spirit, but animated by a very different theoretical background. Apart from the factual support the Wheeler hypothesis is able to muster, it must be confessed that the current scientific mental set is ill-equipped to be receptive to such theorizing. The older sun-spot theory of depressions seems like such a ghastly joke; but now to find not only panics but also the Bill of Rights, Waterloo, the Classical Revival, and Gestalt theory itself traced back to variations in solar radiation seems too good to be true. Perhaps the resistance to any suggestion that our ideas and impulses may not be 'spontaneously' generated, *i.e.*, exclusively 'terrestrially' produced without any derivation from 'celestial' forces, accounts for the difficulty of assimilating Wheeler's spectacular views on the forces behind social change.

INSIGHT AND THE PACE OF CHANGE

Among the many specific issues involved in the general psychology of social change, the one concerning the problem of tempo is perhaps the oldest and most frequently disputed. Does—or should—change occur by sudden 'revolutionary' alteration of the system of folkways or is it simply a cumulation of small, imperceptible, gradual 'gains'? There is no question but that the *ethos* of field theory is more congenial to the

⁴ Dr. George Renner, Professor of Geography at Teachers College, Columbia University, declares that Wheeler's hypothesis is worthy of serious consideration, although he himself has not been able to confirm or endorse all of the specific claims accompanying it. World climate no doubt fluctuates, as the various Ice Ages indicate, but whether it follows a recognizable pattern is debatable.

rapid re-organization of institutional patterns; the emphasis given to the dramatic operation of insight in problem-solving situations strongly supports this view.⁵ All great social events appear to have an 'incubation' phase in which the gradualist aspect is outstanding, but when the movement reaches culmination, its introduction into the behavior-complex of a community has an undeniable mutatory or saltatory quality. The social security and labor legislation enacted during the last decade clearly illustrate this double-aspect theory of social change, for it was preceded by a generation of vigorous Socialist effort in this direction.⁶ The parallelism between the Gestalt theory of learning and its implied theory of social change is all but perfect. In general, much of dynamic social psychology is simply an extension of the wisdom of educational psychologists as this pertains to the growth of the individual.

⁵ Orally, Köhler has declared that the 'suddenness' of the insight phenomenon—so much stressed in the literature—is a secondary rather than a primary characteristic thereof; the important attribute is the new organization of experience.

⁶ Reverting to Wertheimer's commentary on the difference between the old and new liberalism *supra*, it may not be amiss to emphasize that liberalism was gradualist when the world, *i.e.*, the larger social field, was also gradualist. Now, however, liberalism may be forced to quicken its tempo if it is to preserve its nature, for it can hardly take just one step at a time when change on many other 'fronts' (*cf.* the Atomic Bomb) is made by leaps and lurches. When an elevation of the 'electrostatic level' of any total system occurs, it requires a corresponding rise in all absolute positive and negative magnitudes before the leap of potential between them can remain the same. Thus one gets the apparent paradox that for liberalism to maintain its functional equivalence, or 'identity,' in a new era, it must alter its specific content by an addition or subtraction of the parts required, or some comparable transformation.

CLOSURE AND PRÄGNANZ WRIT LARGE

The basic Gestalt principles of *closure* and *Prägnanz* appear to have genuine significance for the process of social change, notably the *direction* that it assumes. These ideas are crucial where *progress*—as distinguished from mere change, which may be retrogressive in terms of human satisfactions—is emphasized. They contain the objective foundations for evaluating events so far as field theory is at present able to provide them. Both *Prägnanz* and *closure* imply that the 'good Gestalt' is the terminal goal of all organizing forces found in nature.

Closure (the less controversial of the two terms) refers to the fact that certain segregated but 'imperfect' wholes tend toward complete or closed forms. Changing, incomplete systems eventually attain equilibrium. In a hyper-nationalistic age such as ours, the easiest social illustration is that form of aggressive group action known as imperialism or expansionism. The Westward growth of the United States was closed after the victorious Mexican War when the sovereignty of the country ended at a 'natural' geographic frontier, the Pacific Ocean. This 'stabilized' America as a major Continental power, its dominion extending from sea to sea. To a map-conscious governing class, the United States as a territorial unit had become a good Gestalt. The British Isles—major land masses sharply bounded by well-defined water barriers—also possessed this important property of more or less final form. By contrast, Germany after 1919 was anything but a good Gestalt—the Polish corridor particularly was established by the victors with no consideration of the persistent psychological tensions thereby created. The fragment of East Prussia and the rest of the Reich 'yearned' to

close the gap separating them. To approximate the feeling of most Germans on this point, an American should assume that Canada had been given possession of all of New York State, thereby effectively cutting off New England from the rest of the country. That the two 'pieces' would make a powerful effort to unite is all but inevitable.⁷

Closure effects may also be discerned in the internal economy of a nation, notably in the drift of most countries toward a collectivist system. Totalitarianism or *Gleichschaltung* is the reorganization of a country in terms of some master purpose emanating from an organizing center. A more closely knit, homogeneous structure results, partly because all non-conformists are exiled or in some way excluded from participation in the compact system. Hitherto, this program has been subordinated to military objectives, but there is no reason why certain types of consolidation would not helpfully reduce much of the bootless pluralism of modern life. Insurance, e.g., would seem to be a natural monopoly, best entrusted to a single giant corporation, ultimately being made a department of the government with premiums collected in tax form, or disappearing altogether in some kind of substituted guarantees of the individual by the com-

⁷ In a strict sense, of course, no *national* unit is a really *good* Gestalt. The United States, e.g., would be a better 'figure' if it embraced Canada, Mexico, and Central America. In an irrational world, it is foolish to minimize this pressure as a factor making for the conquest of smaller nations by larger ones. The ultimate good Gestalt is the World State, completely unified and administered as a single entity. In the long run, an effort to achieve this is highly realistic, although in 1946 it would probably be stigmatized as hopelessly 'idealistic.' Such a planetary commonwealth is the implicit aim of the peace movement everywhere.

munity. Maximum simplicity is one of the results usually achieved by closure.⁸

The related principle of *Prägnanz* holds that all experienced fields tend to become as well articulated as possible. There is flavor of a vague ethical 'must' about this conception as well as an esthetic quality akin to the drive of the artist in his quest for 'the one best way' of doing anything.⁹ Certainly in many high-grade personalities an active interest in social change is due to little more than an educational urge to do better what is now being done somewhat ineffectively. *Prägnanz* may literally symbolize what men on an advanced level often declare is their true function—to bring order out of chaos, or better order out of what is at present poorly ordered.

DIFFERENT LEVELS OF ASPIRATION

A Gestalt social psychology would also be impressed by the possibilities in the Lewinian *Anspruchsniveau* as this relates to public expectations. Many a reformer who has painted a picture of the age of plenty for all (made possible by the miracles of modern technology but lacking only the endorsement of a political plan by the confused elector-

⁸ A number of Nazi students at the University of Berlin in 1930-32 stressed the 'spiritual aid' they felt that their cause received from the principles of Gestalt psychology. The dominance of the whole over its parts seemed to them to support the *Führerprinzip* and many other features of the Hitler program for German regeneration. To the objection that the true 'social whole' was all of mankind, they replied that during their lifetime, the nation or race was the largest *manageable* unit with which they would have to deal.

⁹ Engineers, too, operate according to this over-arching control when they follow 'the most for the least' plan of operation. Gilbreth, who was really an industrial psychologist, made popular the phrase used above. 'Streamlining,' too, is applicable to mind and society, and not restricted to buildings and vehicles.

ate) is often chagrined to discover that the voters are more interested in a 50¢ increase in the weekly relief check now than in an average family income of \$5,000 ten or twenty years ahead. Part of this reaction is due to the greater urgency of an immediately attainable goal, but some of the behavior must be traced to the low level of aspiration prevalent in the population as a result of its many defeats and few successes in the economic field.

The full significance of the level of aspiration as it affects social progress is still to be revealed. Too high a plane of expectation apparently leads to cynicism if unrealized and too low a plane eventuates in apathetic consent to the ills of exploitation and corruption. It is highly improbable that most persons ask more of the social system than it is able to give them. They do not demand the moon, in spite of the fears of those who hold that the anticipations of the masses are not geared to reality. The Townsend Old Age Pension Plan of \$200 monthly for all retired individuals over 65 was probably 'unworkable'

under the then existing economic structure, and its vanishing electoral support suggests that most voters have reached just that conclusion; nonetheless, most readers of these lines properly look forward to an annuity of at least this amount for themselves. Why? Are the class barriers too impenetrable? A full explanation of this would carry us into areas which even the most imaginative of field-theorists have so far failed to penetrate.

REFERENCES

1. BROWN, J. F. *Psychology and the social order*. New York: McGraw-Hill, 1936.
2. HUXLEY, J. Analogies: dangerous and otherwise. *Yale Rev.*, Spring, 1940, 29, 532-555.
3. WATSON, D. L. On the similarity of forms and ideas—as the basis of mental life and of science. In *Scientists are human*. London: Watts & Co., 1938, Chap. 6.
4. WERTHEIMER, M. On the concept of democracy. In *Political and economic democracy* (M. Ascoli & F. Lehmann, Eds.). New York: Norton, 1937, Chap. 17.
5. WHEELER, R. Social behavior patterns and climate. *Soc. Frontier*, 1939, 5, 231-237.

ATTITUDES AND LEARNING: A METHODOLOGICAL NOTE

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Some fourteen years ago the present writer initiated a series of experimental investigations of the problem-solving behavior of the rat which suggested a basic modification of the then orthodox description of the learning process (1, 2, 3, 4). On the basis of the experimental data, first reported in the REVIEW, it was suggested that we could not validly describe the learning behavior of the rat in terms of random responses but instead,

"The animal, in adjusting to a change in his environment (learning), goes about it in an orderly, systematic manner. To the animal any new situation is not a confused, meaningless conglomeration of sensory impressions to which he makes confused, meaningless, uncoordinated and unrelated responses. The animal is not altogether a victim of his immediate environment in the sense that each specific reaction is the result of a specific, momentarily-acting stimulus. He brings to each new situation a whole history of experiences. These experiences the animal is ready to apply. *From the very beginning*, perhaps, the animal goes about solving his problem in a straightforward, comprehensive manner wherein each response is not to be considered as a *Ding an sich* but as a meaningful part of his total behavior. The animal, in executing a series of movements which we call 'perfect,' 'errorless,' 'learned,' 'integrated,' is not doing something which has arisen from a series of 'imperfect,' 'unintegrated,' 'chance' responses. He is now merely running through a different set of integrated responses, which series of integrated responses were preceded by other just as integrated responses" (1, p. 528).

This 'new' interpretation did not derive primarily from new data or a new experimental technique. The experi-

mental situation was the familiar brightness-discrimination box and the obtained data agreed perfectly with similar data collected innumerable times before by various animal experimenters. What was 'new' or 'different' was the method of analyzing the data and the vantage point from which the animals' responses were viewed. This method made it clear that any given 'response' of the animal, when considered as a discrete reaction, recorded as a single statistic to be amassed with similar statistics collected from other animals, and analyzed by an observer biased by the molecular theory of learning, completely fulfilled the expectations of the molecular notions of learning. And, similarly, the same response when seen as a part of a larger segment of the animal's behavior, recorded as a single observation among similar observations made on the *same* animal, and analyzed by an observer biased by the molar theory of learning, completely fulfilled the expectations of the molar notions. Since the data agreed with both types of theories, the latter was given preference because in the opinion of the writer the molar theory could be better defended on other, more general grounds.

Recently the writer has been concerned with some problems in social psychology and a survey of this field has suggested that some of the considerations noted above concerning animal learning might lead to some interesting theoretical treatments and new experimental approaches to the study of attitudes in man.

A popular method of investigating attitudes has been to devise attitude

scales for whatever attitudes we were interested in studying, giving them to a defined group of subjects, and then making a statistical analysis of the responses of the subjects to the items on the scales. Each attitude thus measured was usually regarded as a discrete characteristic of the individual and its meaning was defined in terms of similar responses of other individuals of the group. Thus any one individual's attitude was in the eighth or fifth or second percentile of the group distribution. In one sense this procedure might be called the 'sociological' approach—the *individual's* attitude derived its meaning from the distribution of the attitudes of his fellow men. In the psychological sense such an attitude was treated as a thing in itself and was not too seriously considered in relation to all the other responses of the *specific individual concerned*, to *his* idiosyncratic perception of *his* social world, to *his* needs and frustrations. The resulting conclusions from such a psychologically molecular analysis of attitudes have usually been that people have different attitudes on different subjects, that these attitudes are often internally inconsistent and irrational, that people acquire attitudes and stereotypes as a result of suggestion, propaganda, conditioning and fortuitous experiences which are imposed upon them by outside forces.

Some of the workers in the field of attitudes (notably Gordon Allport) have always stressed the molar approach to the problem and some pioneer studies in that direction have recently been published by Murray and Morgan (5) and Frankel-Brunswick and Sanford (6). Any single attitude, from this point of view, is seen as only one part of a larger segment of the individual's personality, and the meaning of the attitude is derived from the role *this* attitude plays for *this* individual. Such an approach requires a new methodology in

social psychology. If the investigator is to study, for example, attitudes toward labor unions among members of the Republican Party, he cannot stop with identifying his subject as a 'Republican' but he must also examine many other aspects of his subject's personality. Such a program, of course, is a difficult one. Among other things it means that the social psychologist must become familiar with and use the methods and techniques of the clinical psychobiologist if he is to contribute much to the *psychology* of attitudes and not remain content with the *sociology* of attitudes. However difficult, such research might prove to be worthwhile in the resulting new insights which would be gained in the dynamics of attitudes. From the experience gained in studying the rat's problem-solving behavior and the radically different descriptions of such behavior when derived from a molecular approach on the one hand and a molar approach on the other, it might be predicted that we will end up with an analysis which suggests that most people have well-integrated, meaningful attitudes—attitudes which are not imposed on people *ab extra* by blaring and cleverly designed propaganda, conditioning and coercion, but attitudes which are developed by people because of the needs of the personality and the requirements of the situation.

The above argument, it must be confessed, has been argument by analogy from animal studies, biased by a faith in the molar approach to the study of psychological problems. There is, however, a more direct application of the results of animal studies in learning to the investigation of attitudes in people. And it is this application which may suggest a new and perhaps fruitful approach to the study of attitudes.

The concept of attitudes, like so many other psychological concepts, has proven itself as an extremely valuable

one, but at the same time it suffers somewhat from theoretical unclarity and an indeterminacy of its role in the economy of the personality. What is an attitude and what does it do? Most researchers in attitudes agree that one of the functions of attitudes is to integrate social perceptions and experiences. Thus our experiences with our mothers, sisters, girl-friends and maiden aunts are integrated, or combined, or organized (the choice of the word here usually betrays the theoretical position of the writer) into an attitude toward women. Or experiences gained through the press, books, movies, and sermons result in an attitude or stereotype (the choice of the word here usually betrays the social philosophy of the writer) toward the New Deal, the UNO, Communism, labor unions, etc. Another area of agreement is that attitudes are not static; they grow and change and somehow this is due to experience, among other things. Also all writers seem to agree with McDougall that attitudes (or sentiments) involve cognition, conation and affect.

These three characteristics of attitudes—the *integrative* aspect, the *responsiveness* of attitudes to *experience*, and the *cognitive* aspect—almost compel the suggestion that attitudes might logically be regarded as *problem solving attempts*—attempts of the individual to solve some of the problems confronting him in his social world. This formulation would consider the formation, growth and change in attitudes as but special cases of problem-solving behavior. This is not a matter of saying, in more elaborate terminology, that attitudes are *influenced* by learning, but rather that attitudes are the very stuff learning is made of—at least in one social segment of the individual's personality.

There are two implications about the role and function of attitudes which follow from the above formulation. The

first is that attitudes, by organizing and systematizing discrete data and experiences, permit an individual to deal more effectively with innumerable specific items usually subsumed under an attitude. When an individual is presented with new, previously unexperienced social phenomena, he is much in the same position as any organism facing any unsolved problem. After he has systematized these experiences and given them meaning, after he has acquired an attitude about them—or better still, after he has organized them into an attitude—he can deal with those situations in a relatively efficient manner.

The second implication (and perhaps the more interesting one) of the formulation that "an attitude is an attempted solution" is that attitudes have a *means* significance beyond the immediate problem of giving meaning to the data and experiences subsumed under that attitude. The individual in acquiring and changing attitudes is thereby helped to achieve certain *other* social ends. Thus, for example, in acquiring certain attitudes toward the CIO or Communism or the Negro, the adult is frequently aided in achieving certain desired group-membership or group acceptance; or the child is able to escape censure and even punishment. Or the adoption of an attitude identifies us more closely with desired persons or ideologies; or the attitude will seem to make easier the solution of urgent social, political and economic problems.

A word of caution: The above formulation does not imply that every attitude is *consciously* adopted as a means toward achieving some other end. In other learning processes hypotheses are frequently adopted without conscious awareness. What is implied is that attitudes which are adopted do have a 'means' function. Nor does the above formulation deny the possibility of attitudes acquiring functional autonomy.

It is commonplace to find means becoming ends.

The above formulation immediately suggests that we apply to the study of attitudes the theory and techniques of the psychology of learning. Such problems as the responsiveness of attitude changes to changes in the needs of the person and the social environment become parallel to the problems of the docility of means-end-readinesses (to use Tolman's terminology) and we may expect to find that the laws governing the latter also apply to the former. The problem of the fixation of attitudes immediately falls into its place among similar problems investigated in the field of learning. The problem of the *history* of an attitude also becomes a special case of a recognized problem in learning theory. The experimental and 'theoretical' psychologist need not drop his techniques and principles acquired in the investigations of learning when he approaches the study of man and attitudes. Are attitudes the result of hit-and-miss, trial-and-error associations? Or will an investigation of the development of attitudes show that the attitudes of man, no less than the hypotheses of rats, display a meaningful, organized development? Will we find, if we make a serious effort to study the developmental process of attitudes as a learning problem, that man, in trying to solve some of his social relationships (attitude formation), goes about it in an orderly, systematic manner; that to man any new situation is not a confused, meaningless conglomeration of social stimuli toward which he adopts confused, meaningless, uncoordinated and unrelated attitudes; that man is not altogether a victim of his environment in the sense that each specific sentiment or attitude is the result of a specific, momentarily potent stimulus; that from the very beginning of his existence as a social animal man regards his society in an organized manner

wherein each attitude is not to be considered by itself, but as a meaningful part of his total behavior; that man, when and if he finally arrives at what the arbitrators of society call an integrated or desirable or acceptable set of attitudes, is not displaying something which has arisen from an unintegrated series of discrete attitudes, but that he now merely has a *different* set of integrated attitudes which had been preceded by another just as integrated system?

If our analysis of the problem-solving behavior of the rat has any merit at all, and if the concept of attitudes as 'attempts at solution' is a valid one, we are not being overkind to man in daring to suggest that a correct description of the formation of attitudes will be in terms somewhat like those outlined above. This is not a call for a religious revival of faith in the rationality of man. We believe that the above description is a possibility which derives from an objective consideration of experimental data applied to a functional concept of attitudes. A cold, sober, scientific analysis of man is not synonymous with a description of man as a chaotic, senseless, irrational aggregate of stereotypes, prejudices, biases and conditioned responses fortuitously acquired.

REFERENCES

1. KRECHEVSKY, I. 'Hypotheses' in rats. *PSYCHOL. REV.*, 1932, 6, 516-532.
2. —. 'Hypotheses' vs. 'chance' in the pre-resolution period in sensory discrimination-learning. *Univ. Calif. Publ. Psychol.*, 1932, 6, 27-44.
3. —. The genesis of 'hypotheses' in rats. *Univ. Calif. Publ. Psychol.*, 1932, 6, 45-64.
4. —. The docile nature of 'hypotheses.' *J. comp. Psychol.*, 1933, 15, 429-443.
5. MURRAY, H. A., & MORGAN, C. D. A clinical study of sentiments. *Genet. Psychol. Monogr.*, 1945, 32, 3-309.
6. FRENKEL-BRUNSWIK, E., & SANFORD, R. N. Some personality factors in antisemitism. *J. Psychol.*, 1945, 20, 271-291.

COMMUNICATIVE BEHAVIOR

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Communicative behavior, one of the fundamental varieties of interaction at the level of perception and mental manipulation, underlies and makes possible collective and corporate behavior (5, 6, 7). Without it no social group could exist; human society would be impossible. In its absence no voluntary, intentional sharing of experience by two or more people would ever occur.

Communicative behavior in its simplest reciprocal form is the use of some action by one person, whether or not accompanied by a material object, as a stimulus to another person in such a way that the second person can perceive the experience of the stimulating person. The overt action of the first person plays the role of a symbol whose reference or meaning is the same for the two participants, with the result that common experience is perceived by both the participants.

Several important facts about the nature and varieties of communication require discussion. In the first place, communication does not involve conveyance or transportation of material objects; rather it is a process of arousal and guidance of the attention and perception of those communicated with. It depends on some common basis of experience in the communicating people. The mutual understanding thus arises from perception of the same or similar experience. The responder in communication never merely imitates another. Parroting the actions of another, mere memorizing of words, is not reproducing the meaning of the words. The communion is more fundamental than the stimulus given by A to B. If true communication occurs, it is as a result of a translation by B of the

stimulus of A so that the perceptions of A and B are approximately or exactly the same.

All of the psychological phenomena of people enter most examples of communication. Sensation is implicit in all response. Perception of the actions of others is necessary also. But behind the perception of the action also lies the perception of its meaning, of what it signifies, both to oneself and to the actor. Such perception involves imagination, and often logical analysis and inference to a much greater extent than it does unchanged memory images. Intuition—by which we mean that intermediate psychological steps are left out at the moment (although originally present), and conviction and understanding appear to be obtained immediately—also enters in most cases. Imagination is, however, probably the most important element of all. The recipient of language stimuli must put himself in the place of his communicant, must see things through his eyes, must play his role, before he can fully understand him.

When anything more than a superficial examination is made of the nature of communication it is immediately recognized that there are various degrees or levels (1, 9) of communication, the differentiation of which is most important. The total experience of a communicant or only some part of it may be perceived by a second communicant. Possibly the lowest level or simplest part of the total experience that may be communicated is the fact or act of attention. The communication of one's attention to some object by pointing or uttering a sound or the indication that one feels a powerful emo-

tion is the simplest degree of communicative behavior. Transmission of objective information is also on a rather simple level, for example, information about number, size or direction of some object not immediately visible. A further degree is exhibited in cases where one person succeeds in sharing an exact experience of excitement, wonder, anticipation or awe with another. Still further levels involve common perception of systems of value, attitudes, desires and decisions. Complete communication is obtained in part by interchanges in which B attempts to indicate what he understands and A embellishes his own activity or suggests alterations in B's statements with the belief that common understanding will be enhanced in this way, in part by each person trying to reproduce the other's experience. Neither participant is content with observing common media of communication (words, inflection, rhythm, gestures) but tries to obtain the same reaction to the media, that is, have the same experience of them.

Personality traits and individual differences affect the ability to carry on complete communication. Many people are not able to engage in complete communication, because of mental dullness or inertia (perseveration). Large numbers are incapable of sufficient imagination to do so. Others are too ego-centric and are bent continually on self-expression, with the result that they never imaginatively play the roles of others with whom they are associated. Some people play thought roles of others, but stop short of emotional ones. This may be a serious handicap to understanding, because of the importance of sympathy for complete comprehension of another person's mental patterns and actions. Often it will be observed that conversations are in reality two monologues that occur serially without either person actually understanding the

other. Aggressive personality traits tend to crowd out true understanding. The neurotic person often is too much occupied with his own emotional difficulties to comprehend another. One who is introverted or otherwise accustomed to social isolation often finds it difficult to play another's role. And those who are in such a hurry that they have little time for interaction usually show little ability to understand others in the fundamental sense implied. The hurry of modern life had made complete communicative understanding of others somewhat more difficult than formerly.

For interaction resulting in agreement and consensus among persons with freedom of choice, the importance of communication is even greater. When people are about equal in influence on each other and have had similar experiences in dominating, they tend to come to decisions on the basis of free interchange of points of view, perceptions, attitudes and opinions. Such activity demands imagination, tolerance and willingness to look at the matter under review through the eyes of others. It is therefore less common than leadership-fellowship and domination-submission. However, imaginative role playing and 'looking at both sides of a question' are time-consuming and even they do not always result in agreement. Pressure of time and of danger arising from prolonged indecision, and the forcefulness of intolerant and selfish persons, at times wreck attempts to use the full deliberative method of complete communication among equals. Yet such friendly discussion provides the most perfect example of behavior known to human beings in which equality of status and individual differences are fully respected and where concerted action takes place among equals after consensus.

Domination, aggressiveness and ascendancy, although rarely resulting in

complete understanding, do not preclude quasi-communication. They are dependent on one-sided communication of desires and intents of a vigorous, speedy and explicit nature. The chief means of such influence is an ordering and forbidding technique which inhibits the exchange of information and particularly of insight into the mental and emotional patterns of those who are subject to the orders and restrictions. In other words, provision is made for one-way understanding but not for complete communion. And even the unilateral form comes to exist only with reference to objective events and impersonal facts, since the separation of the two types of people is so great that there is little comprehension on the part of either participant of the mental habits of the other. This is exemplified in all caste systems and in all relatively rigid class hierarchies.

It may be suggested that the importance of complete communication is least in such interaction as that between free strangers, that which involves opposition and that between dominator and subject. On the other hand, complete understanding is of the greatest importance in voluntary rational consensus between equals or near equals in which the desires of all are merged with as much toleration and as little violation of individual motives as possible. Intermediate degrees of understanding are required when the persons are no longer free or strangers, not strongly opposed to each other, or where the status and influences are neither completely similar nor completely different.

The essentially voluntary nature of complete communication is most obvious in the fact that each communicant must be in control of his stimulus behavior. In quasi-communication, when one's ideas are apprehended without his cooperation or consent, the understand-

ing is one-sided. But in complete communication each person controls and selects the stimuli he is using to make clear the meanings to be communicated. Conveyance of accurate information about objective phenomena and about one's feelings illustrates what is necessary. In such cases the selection of certain gestures and words is needed by each, if the stimuli of A are to be understood by B and A is to be certain that B understands. Of all the possible stimuli each may employ those apropos to the immediate interaction must be selected, if mutual understanding is to result.

Further light may be derived from a consideration of examples of partial communication. The child at first does not respond to other people, but only to his own inner state or to general external stimuli. He also has no ideas and is unable to act in anticipation of the responses of other people. But the child's actions are interpreted with some success by others who must care for him. The latter come to distinguish 'hunger discomfort' from 'chafing discomfort.' Without intent the child cries and the locus of the 'cause' or at least the 'symptom' of the disequilibrium must be inferred without much help from him. The adult has to make practically all of the adjustments and use all of the insight and sympathy.

Other examples of partial communication are those in which a person betrays experiences or intentions that he prefers to conceal. In many cases he is unconscious of giving away any clue to his internal state. But his face is mobile, his voice reflects his emotional state, he exhibits tics or tremors, he rubs his head, pulls at his ear, scribbles on the telephone pad, whistles or hums a tune (3). Whatever is observable about him in relation to the external world may be interpreted by others and hence become a part of an incomplete

communicative act, since the person exhibiting ideomotor phenomena may not be aware that others have observed him.

Many people employ observations of other people's ideomotor action to their own advantage. Athletes use them to place opponents at a disadvantage, since the kind of pitch or the ball carrier in a football scrimmage is often unconsciously revealed to an opponent. Lawyers, social workers and diplomats also learn to understand their associates through shrewd observations of hands, voice, eyes, posture and other subtle phenomena. But more than any other occupation 'mind reading' depends on such quasi-communication. Conversation reveals much more than the speaker intends, and the palmist has the advantage of being able to feel the tremors of the client which reveal his emotions (8). Even animals can react with great discrimination to subtle signs.

The interaction of young children on a pre-language level also reveals important facts about true communication. Nursery school children carry out social behavior on a primitive muscular basis, reacting to others as part of their environment and to the responses of others to their own acts. They also apply to others the forms of action shown toward them. For example, B reaches for a doll that A has. A draws back. B tries again, giving evidence of anger (makes sounds, frowns). A again withdraws and looks at B. Now when another object (a block) is presented so that each can obtain it, B takes it while A looks on. Then A gives the doll to B who gives the block to A. B tries to take the block back but A withdraws and then they complete the exchange. B appears to gloat over getting the block while A looks on. When another object is shown both children, B again takes it but offers block to A. B makes 'expressive' sounds at A and shows the

block to A who gives no more heed to B (2). It is clear that here the responses of each infant change the situation as the other perceives it. It is also clear that each reacts to the beginning of the other's response as when B reached for a toy that A had possession of. When the beginning of a movement is responded to by each child as if it were a complete movement, we may infer that each child imagines its own complete act and even the combined act of both. On the muscular level interaction by means of partial acts, or gestures, is called by Mead a 'conversation of gestures' (4), and is the beginning of true communication.

At this level we cannot be sure how completely each child understands what the other perceives, but the perceptions are probably vague in infancy. Indeed, it is not until considerably later that full communion between experiencing people is possible, and then it will be made possible by well-differentiated symbols. Not until the person consistently uses an action to express an experience and uniformly translates that action by another into that experience is he prepared for complete understanding of another's perceptions. And not until two people have common understanding of the same observable sign or symbol can they communicate in the fullest sense of the term.

It will be of further assistance in understanding communicative behavior to examine the extent of its occurrence. Communication, dependent as it is upon perception of the same meaning, the same significance of the same symbol, by two or more organisms, is exclusively human, although such animals as 'talking horses' seem to have slight ability to use symbols and to carry on at least quasi-communication, and such humans as very young children and idiots are unable to participate in communicative behavior.

It does not follow, however, that all human beings carry on communication whenever interaction occurs. For example, in athletics and in fighting there are many occasions when communication by means of symbols is not only impossible but discouraged, although 'psychological warfare' and taunts are commonly used to interfere with the effort of the opponent. Training for combat and athletic activities generally involves the deliberate suppression of full understanding of the experience of the opponent, and a fixed attitude of antagonism, or indifference toward the general desires of the opponent, but the cultivation of every talent for perceiving the specific method the opponent will employ in carrying out his desires.

It is also easily understood that although communication appears to be limited to human beings, not all men and women are able to communicate the same kinds of experiences or ideas, because not all human beings can perceive all kinds of experiences or ideas, and not all kinds of human beings can learn or have learned to control and use the actions that have the power to stimulate others meaningfully. Musical phenomena are not universally perceived and therefore cannot be communicated to everyone. Nor can all who experience great music express it with voice or instrument and therefore engage as agents in communicative musical behavior. Scientific language and mathematical language are equally good examples. An overwhelming majority of Americans are untrained in all of these special languages and are therefore unable to communicate with people who are experienced in the use of such symbols. There is almost as complete a cleavage between the minds of ordinary high school graduates and the participant in the most advanced scholarly communication as between the ordinary man and the more intelligent ani-

mals. A definite effort is required by the abstract scientific worker to communicate with a high school student without using words that will not be understood, and the habitual manipulator of abstractions may have difficulty understanding the language of the high school student, who has also developed or acquired a partially independent language of the athletic field and the world of slang.

The relationship of communication to other forms of social behavior also deserves attention. Two fundamental qualities of social behavior are its degree of freedom and its degree of associativeness, each of which provides for a continuum of degrees. In the former this continuum is between the theoretical extremes of complete freedom and complete control, and in the latter the continuum is limited by completely associative and completely dissociative behavior.

Completely free social behavior is characterized by interaction in which neither participant obtains an advantage over the other during the interchange, and following which there is no differential retention into the future of a tendency to influence the other. In free social behavior the interaction may have changed each participant in important ways, but not in such a way as to leave either with a set toward control by the other greater than before the interaction. Controlled behavior, on the other hand, is behavior in which one participant influences the other more than he is influenced by the other. The freedom of the controlled participant is interfered with by the controller, with the result that a tendency toward future control has been created or strengthened.

It is difficult to discover a point of reference from which to make a satisfactory determination concerning the degree of freedom of participants in so-

cial behavior. But it is easy to decide that there is no necessary relationship between communication and the qualities of freedom and control of individuals engaging in social behavior. Communication may take place between free and equal people without producing controlled behavior. On the other hand, when control relationships exist communication may occur without changing the control relationship. In still other instances communication may take place in such a relatively one-sided way that the balance of influence between the participants is changed. If one person dominates a general conversation and at the same time inhibits others' attempts to lead the conversation and entertains most of the listeners, his control over the other people may be greatly heightened. Here, however, freedom does not actually exist in the situation, although the individuals may be relative strangers. In other cases a control relationship may be reduced by communication, as when people of different ranks in a relatively rigid hierarchical organization have the opportunity to associate informally and the ideas of the person in the inferior status so impress the person in the superior status that relationships of greater equality result.

Communicative behavior may also be related to the free and to the controlled variety as a preliminary step. Although behavior identified as free and as controlled may be carried on independently of communication, the perception of desire to communicate, of probable successful stimuli to the other person and the corresponding perception by the respondent of the initiating person's activity and meaning are prerequisite to a response by the respondent which, in the case of free behavior, will be meaningfully related to the initiator's behavior, and which, in the case of controlled behavior, will be made

in accordance with the behavior which the initiator desires the respondent to carry out but for which no already existing cues and cue-sets exist. That is to say, communication is a preliminary step in a complete communicative-controlled act when the control involves a new adjustment between initiator and responder, which, in turn, is dependent on mutual understanding. In such cases, very frequent in a changing social milieu or one in which there is much mixing and moving of individual people and groups of people, communication is not only preliminary to but is also prerequisite to the development of controlled behavior.

The relationship of communicative behavior to associative and dissociative behavior is somewhat more simple. As already indicated in reference to fighting interaction, communication between opponents, each of whom is acting dissociatively toward the other, is unilateral and used for the entirely dissociative ends of destruction or exploitation of the opponent. In short, communication between opponents is not necessary and where it occurs is kept subordinate to the dissociative goals of the opponents. If it is not kept subordinate to the dissociative goals, it tends to reduce the degree of the dissociation. Thus fraternization is prevented by victorious armies, if they desire to retain the pre-fraternization relations of superiority, ethno-centrism and prejudice. For full communication is essentially conducive to associative relations of all sorts. It makes possible the sharing of experience. It involves each participant attending to and responding to the other in major or minor ways. It provides each participant with an opportunity, and also makes it necessary for him to play his part as a person of importance, to make his contribution, if not as initiator or leader, at least as a respondent whose

response helps to satisfy an initiator's desire. Since it contributes to the mutual satisfaction of the participants' desires, communication tends to develop friendly relations and protagonistic attitudes.

Communication, involving the sharing of meanings and the interchange of stimuli and responses, is indeed a form of coöperative behavior, although not coextensive in reference to all kinds of coöperative behavior. Communication is coöperative in the sense that behavior antagonistic to the other participant is inhibited by each participant during the communication act, while behavior friendly to the other participant is facilitated. Participants act together to facilitate mutual understanding. However, communication must be distinguished from muscular coöperative behavior. It is used as a preliminary step to coöperative muscular behavior, as in the communication of football signals before the scrimmage play begins, or the planning of the conduct of a trial defense before the trial begins. Communication is also a constant accompaniment of most activities of muscular coöperation.

Communication is also prerequisite to muscular forms of coöperation. Without the sharing of experiences necessary to the coördination and synchronization of effort coöperative endeavor would be impossible. Constant training in coöperative muscular behavior tends to reduce somewhat the necessity of communication to the execution of the coöperative activity. However, even in relatively simple coöperative muscular activity some communication, both bilateral and unilateral, takes place. The orchestra conductor supplements the extensive practice sessions with gestural cues to coördinate the performance of the musicians. Tennis players communicate while at play, and the members of a baseball team are in more or

less constant communication in both offensive and defensive maneuvers. It would be impossible to find any example of prolonged coöperation by even two people that does not require communication on at least a unilateral basis.

The role of communication in collective behavior, both the unincorporated and corporate varieties, is not essentially different from its role in coöperative interaction. The collective relationship marked by perception of membership in a collectivity (6) may develop without communication through formal language, but it is dependent on common interpretation of the meaning of overt behavior to the participants. The adjustment of the activities of the participants in order to progress toward the common goal is also dependent on the perception by each of the emotional experience and desire of others constituting the collectivity. Interchange of more or less subtle stimuli and the perception of their meaning are constant in all collective situations. In other words, collective behavior is preceded by, accompanied by and is dependent upon communication.

The importance and role of communication in corporate behavior differs little, if at all, from what it is in unincorporated collective behavior. Since "participants in corporate behavior are psychologically inseparable and act as one unit with reference to the surrounding situation" (7), it may appear that communication is not so important as in other coöperative behavior. However, because biological incorporation of human beings does not occur and all cannot be stimulated directly as parts of a single nervous system, it is obvious that the role of communication is generally the same for all coöperative behavior. The only important difference between the role of communication in corporate and unincorporated collective

behavior is that in the former communication is not required in order to develop the perception of membership in a collectivity; the corporate unit's members already have a constant background awareness of their membership character in the group. In both forms of collective behavior communication retains its properties of providing for mutual understanding and communication of all the levels of perception and experience.

REFERENCES

1. COOLEY, C. H. *Human nature and the social order*. New York: Scribner, 1902.
2. HILLER, E. T. *Principles of sociology*. New York: Harper & Brothers, 1932.
3. KROUT, M. H. Autistic gestures. *Psychol. Monogr.* 1935, 46.
4. MEAD, G. H. The mechanism of social consciousness. *J. Phil.*, 1912, 9, 401-406.
5. SMITH, M. An approach to the study of the social act. *PSYCHOL. REV.*, 1942, 49, 422-440.
6. —. Social interaction and collective behavior. *PSYCHOL. REV.*, 1944, 51, 127-135.
7. —. The psychology of the corporate act. *PSYCHOL. REV.*, 1945, 52, 100-108.
8. STRATTON, G. M. The control of another by obscure signs. *PSYCHOL. REV.*, 1921, 27, 301-314.
9. ZNANIECKI, F. *Social actions*. New York: Farrar and Rinehart, 1936.

THE HEREDITARY DIFFERENTIAL IN LEARNING— A REPLY TO F. A. PATTIE

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In a recent criticism of experimental findings originally reported by D. O. Vine and the writer (2), and more recently interpreted by the writer (1), Frank A. Pattie (3) proposes that the reported hereditary differential between bantam and leghorn chicks in learning to go to a chick of their own kind for food rather than to one of the other kind, when training influences toward both kinds are presumably equal from birth, is really due to environment rather than heredity.

In the cited experiment equal numbers of bantam and leghorn eggs were hatched at the same time in the same incubator, and thereafter equal numbers of each kind of chicks were reared together in a brooder until three weeks old. They were then trained in a Y choice box to be guided to food by a certain other chick (cue animal) which was caged near it. For certain chicks the cue animal was of the same kind; for others the cue was other kind. The alternative alley provided electric shock, indicated by the alternative kind of cue animal. During the course of the experiment all chicks served both as learners and as cue animals, and learned in sequence or reverse sequence to go for food both to their own and the other kind of chick. Finding was that the chicks, in general, learned to go to their own kind more quickly.

Pattie suspects that because the leghorns were larger they may have developed a 'despotism' (or dominance) over the bantams, especially during the three weeks they were all together in the brooder. The concomitant of this dominance of leghorns would, of course,

be fear (or submission) on the part of the bantams. As a result of this fear the bantams would presumably take longer to learn to go to food when it was near the leghorns (other kind) than when it was near bantams (same kind), as appears in the experimental data. In this instance, at least, he believes the reported greater facility in learning to go to own kind "can thus be explained by the hypothesis that non-hereditary factors produced the differences found." The fact that a smaller (statistically unreliable) difference of this kind was found with leghorns as runners tends, the critic thinks, to bear out his hypothesis.

Pattie says that he 'does not take issue with the definition of heredity above quoted,' that it be arbitrarily defined as 'the innate differential in development.' With reference to the greater size of the leghorns and its consequences in learning, the writer would like first to ask whether or not this differentiation in size did not grow out of innate differences in the content (nuclear and nutritional) of the eggs at the time of hatching. Except for these eggs, the environment was impartial and could not of itself differentiate development. Even though each kind of chick constituted a different kind of environment for the other kind than the other kind did for it, yet this 'environmental difference' actually originated in the eggs, because the same air, food, water, etc., were provided for both kinds. It should be obvious that innate or hereditary differences cannot remain always separate and unmixed with environment; their later effects on development

are necessarily achieved through differences in the way in which the originally common environment is used, or reacted to, or rather interacted with. The idea that hereditary effects are necessarily independent of physical and environmental means must be abandoned.

Even if it is granted, therefore, that the larger size of the leghorns may perhaps have induced more fear, and therefore more avoidance of unlike kind than of like kind on the part of the bantams (a phenomenon not actually observed by the experimenters), it is still not evident that this differentiation was not originally innate, or 'hereditary' in the defined sense.

Aside from given specific or unidirectional factors, however, such as the one suggested, there still remains the problem of whether there may or may not be a confluence of various other, perhaps unknown, causes of differentiation, so numerous, possibly, that their effect is bidirectional as far as interaction between different kinds of animals is concerned. The critic apparently believes that such effects are not demonstrated by the experimental report, evidently because the bantams showed more tendency to go to their own kind than did the leghorns. Since the margin of tendency of leghorns to go to their own kind rather than unlike kind was not, of itself, statistically significant, he believes that it is probable that a smaller margin would be found if the experiment were repeated; and that consequently the combined data, indicating greater readiness to go to like kind in general, are unreliable.

It seems evident in this criticism that the critic has failed to take account of a planned set-up of the experiment (described in the report) which tends automatically to compensate in the general summation for such one-sided effects as the one he hypothesizes. While he postulates a despotism-fear relation-

ship between bantams and leghorns, he actually considers its effect upon the bantams only; he overlooks the fact that the leghorns would also be influenced by this mutual relationship—but in reverse direction, so that as a result they would tend to go to unlike rather than to like kind, thus reducing the difference in their reactions to kinds of cues (as appears in the data), and thereby, incidentally, also the magnitude of the total difference in tendency to go to like and unlike kinds which appears in the general summation. If during the pre-training period, the bantams learned to yield or avoid taking food from the leghorns, the leghorns would correspondingly learn to dominate, go to and take food from bantams more than from their equals, the leghorns. The effect of such a despotic relationship, therefore, on the one hand, would be to reverse, or at least reduce, the tendency of leghorns to go to like kind, even while, on the other hand, it increased this tendency in the bantams. If such relationship were eliminated in the experiment, therefore, the tendency of leghorns to go to own kind would probably be increased, rather than reduced as suggested by the critic. In this event, however, the greater tendency of bantams to go to like kind would be reciprocally reduced, so that the average for both kinds as presented in the final summation should remain about the same.

For these reasons, therefore, as explained also in less detail in the experimental report, unidirectional factors tend to cancel out in so far as their effect on the total result is concerned. Thus it seems reasonable, as well as statistically justified, to assume that the final summation is more significant than any of its components taken alone.

The warning that one should be cautious about accepting statistical averages blindly, without examining the

elements, is of course justified. Nevertheless, it should also be recognized that one can err just as much in under-rating the significance of a statistical average as in over-rating it. Extreme statistical agnosticism is actually no more virtuous than statistical gullibility. Cold, calculated estimation of probabilities, neither optimistic nor pessimistic, is just as necessary for the scientist as for the gambler. In terms of chance only, any other obtained difference between averages is just as likely to be more than the observed difference as to be less. Also, as in the present instance, composite averages are normally more reliable and dependable than their component measures. This is the reason that winning sets in tennis is accepted as better evidence of superiority than is winning serves or games. Smaller statistical reliability of component groups of data (pointed out in the criticism) is not disproof of the otherwise attested reliability of a summation, especially if the critical ratio is over six as in the study under consideration.

If time were available, it would of course be desirable to carry out the critic's suggestion that the experiment be repeated with the fowls isolated from one another during the pre-training period, rather than allowing them to mix freely as they did. Perhaps different results would be obtained, but it is doubtful that they would be more valid. Extension of this logic of prevention of learning as a means of discovering hereditary factors, moreover, would seem to demand that training in the choice box be eliminated also. As suggested in the original report, it is probable that various unidirectional or biasing factors (*i.e.* differences between kinds in size, brightness of color, activity, noisiness, intelligence, etc.) operate in the choice box as well as outside of it. This experimental technique of restraint is what we wanted to get away from,

since it tacitly applies the false traditional criterion of heredity, that its effects are manifested in the absence of learning, rather than by differentiating the process of learning, as proposed by the writer. If prevention of learning is effectively carried out, it appears to the writer that it is almost a certainty that no hereditary effects at all will be found. This isolation logic is admirably adapted to prove a case for environmentalism, since, if rigorously applied, it conceals every hereditary effect. If chicks were kept isolated until old age, they would probably not differentiate responses to own and other kind, even with respect to sex.

Looking outside the study under discussion, with respect to the more general problem of whether or not there is a hereditary differential in learning, attention should perhaps be called in this reference to certain additional evidence presented in a study by J. B. Schoolland (4), published since Howells and Vine's report. The general method of this experiment is similar to that of Howells and Vine in that equal numbers of fowls of two different kinds, chiefly ducks and chickens, were placed together soon after hatching in incubators, and reared together until placed in various experimental situations, in order to test differences in behavior tendencies which developed in the two kinds of neonates in a common, non-differentiating environment. In general, the findings are similar to those presented by the writer.

An experiment described on pp. 257-269 of Schoolland's study is especially significant with respect to the present discussion because of both similarities and differences of the procedure as compared with Howells and Vine. Chicks and ducklings were put individually in cages which were placed adjacently to each other, like the cut pieces of a pie, so that together they constituted a

circle. The order with respect to kind of fowl was an alternation of pairs of the same kind (duck, duck; chick, chick; duck, duck; chick, chick), so that the circle was completed with eight fowls. The strategy in placement was to insure that each fowl had a fowl of the same kind on one side of it, and a fowl of the other kind on the other side. Partitions between the cages were of wire screen, so that each fowl could see and hear adjacent fowls.

The first step in procedure was to shock half of the fowls of a given kind, and photograph resultant movements of all fowls (like and unlike, shocked and non-shocked) with a motion picture camera which was mounted above the center of the circle, after which the same procedure was repeated with shock applied to half of the fowls of the other kind. Following this, all of the fowls were simultaneously shocked. Next, half of the fowls of one kind were again shocked and reactions photographed, and again all of them together, in alternate order throughout the experiment. The purpose of shocking half of the animals of one kind was to observe the comparative effect of their noises and movements, as social stimuli, upon the adjacent fowls, like and unlike, which were not shocked; while the purpose of simultaneously shocking all of the fowls was to provide each fowl with an adequate, unconditioned stimulus for jumping and crying: and at the same time to

associate with this shock stimulus the sight and sound stimuli from the different fowls on either side, so that each fowl had an equal opportunity to learn or become conditioned to such social stimuli irrespective of whether they came from his own or the other kind of fowl.

As the experiment continued for ten days, there was a progressive increase in reactions to social stimuli as provided by all fowls, but especially to those of like kind. Instances of greater activity response to stimuli from like kind, as compared with those from unlike kind, were in a ratio of 245 to 120 (as tabulated by the writer).

The fact that results in Schoolland's experiment were similar to Howells and Vine's results, even though a different combination of fowls was used, and that no food reward was involved, would seem to minimize the possibility that the findings of either experiment could be fallacious.

REFERENCES

1. HOWELLS, T. H. The obsolete dogmas of heredity. *PSYCHOL. REV.*, 1945, 52, 23-34.
2. —, & VINE, D. O. The innate differential in social learning. *J. abnorm. & soc. Psychol.*, 1940, 35, 537-548.
3. PATTIE, F. A. Howells on the hereditary differential in learning—a criticism. *PSYCHOL. REV.*, 1946, 53, 53-54.
4. SCHOOLLAND, J. B. Are there any innate behavior tendencies? *Genet. Psychol. Monogr.*, 1942, 25, 219-287.

The first of these is the fact that the American Medical Association is a voluntary association of physicians and surgeons. It is not a government agency, nor is it a religious organization. It is a purely professional organization, and its members are bound by the same rules and regulations as any other professional organization. The second fact is that the American Medical Association is a non-profit organization. It does not have a profit motive, and its funds are used for the benefit of its members and the public. The third fact is that the American Medical Association is a democratic organization. Its members have the right to elect their representatives to the governing body, and they have the right to be heard in their own defense. The fourth fact is that the American Medical Association is a scientific organization. It is based on the principles of science, and its members are committed to the advancement of medical knowledge and the improvement of medical practice. The fifth fact is that the American Medical Association is a humanitarian organization. It is concerned with the welfare of the patient, and it is committed to the highest standards of medical ethics. The sixth fact is that the American Medical Association is a patriotic organization. It is devoted to the service of the United States, and it is committed to the principles of democracy and freedom. The seventh fact is that the American Medical Association is a progressive organization. It is open to new ideas and new methods, and it is committed to the improvement of medical practice. The eighth fact is that the American Medical Association is a cooperative organization. It works with other organizations to achieve its goals, and it is committed to the betterment of the medical profession and the public. The ninth fact is that the American Medical Association is a responsible organization. It is aware of its duties to its members and to the public, and it is committed to the highest standards of responsibility. The tenth fact is that the American Medical Association is a successful organization. It has achieved many of its goals, and it is committed to continuing its work for the benefit of the medical profession and the public.

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